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Epistemic Decision And The Duhem Problem

Po Keung Ip

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EPISTEMIC DECISION AND THE DUHEM PROBLEM

by
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Submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy

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ABSTRACT

The Duhem problem, as interpreted here, is concerned primarily with the ambiguity of scientific testing---falsifying experimental data can never uniquely determine the theoretical target to be falsified. By reformulating the Duhem problem within an epistemic decision framework recently proposed by Isaac Levi, it is hoped that the Duhem problem can be seen as a problem in epistemic decisions and hence can be solved decision-theoretically.

Our discussion will begin by first examining some of the motivations for adopting the decision-theoretic approach to scientific inquiry. In chapter 2, we shall review how some sense of decision is shared among contemporary thinkers in scientific methodology. Specifically, we shall discuss how the choice of protocol sentence is viewed as a kind of decision. Views of Popper, Carnap, Lakatos, Hempel and Levi are reviewed. The structures of practical decision and cognitive decision are critically compared. Moreover, epistemological concepts relating to epistemic decisions---contextualism, corrigibilism, epistemological infallibilism, local methodology---are discussed in chapter 5. An analysis of the Duhem problem with reference to Duhem's and Quine's work, and major ways to combat the Duhem problem are presented in chapters 3 and 4. In chapter 6, a detailed and critical exposition of Levi's epistemic model

is given.

The final chapter is our decision-theoretic treatment of the Duhem problem. The Duhem problem is interpreted as a problem in epistemic decisions. More specifically, the problem is transformed into a problem of choosing alternative corpora relative to corpus contraction. Some measure of the utility of the corpus is proposed. The utility of a corpus is understood here as the cognitive performance of the corpus which is defined in terms of its problem-solving (question-answering) capability relative to a domain of problems and cognitive objectives. Indeed, by utilizing the measure of corpus performance, we can provide a ready solution to a version of the Duhem problem.

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CHAPTER I

INTRODUCTION

The primary objective of this dissertation is to show that the Duhem problem can be seen as a problem in epistemic decisions and hence can be tackled decision-theoretically. Though there are many versions of the Duhem problem, we shall only attend to one widely accepted interpretation of it. Namely, the Duhem problem, as understood here, is a problem concerning the ambiguity of scientific testing---falsifying experimental data (negative evidence) can never uniquely determine which part of the theory complex is to be falsified.

Discussions on the Duhem problem in the literature usually, implicitly or explicitly, presuppose that the problem is a problem of inference. That is, given a negative result in a scientific test, how can we know, by inference, which one of the members of the theory complex (specifically, the hypothesis under test and the auxiliary hypotheses) is responsible for the false prediction? Indeed, seen in this way, the problem presumably resembles the problem of justification in an inductive argument. That is, the problem of justification of a conclusion which is not deductively entailed by the body of evidences. Alternatively, the problem can also be viewed as one which concerns the acceptance or rejection of a portion of a

theory complex on basis of a body of evidence. In the Duhemian case, the evidence here refers to the negative consequence of the test, and the question is what part of the theory complex is responsible for producing it. In other words, in situation like this, what portion of the theory has to be rejected? It can be readily seen that being so interpreted, it does have an argument structure very similar to that of an inductive argument. At the same time, however, it is also vulnerable to the challenge raised by Hume to the effect that no conclusion of an inductive argument can ever be justified.¹

Is there, then, any inductive rule that could overcome the Humean challenge? That is, given the negative instance, could we inductively infer, via some rational rules, which element is to go? According to a widely held view of inductive inference, inductive rules should not be seen as assigning specific inductive conclusions to a given body of evidence, but rather as principles specifying a certain probability between a hypothesis and a given body of evidence. Such an inference, however, does not allow us to argue from evidence e to hypothesis h. Instead, it only specifies the probability of h on e: $p(h, e) = r$. But such an interpretation is also defective in another aspect. According to some, in science as well as in everyday life, we do need to accept certain hypotheses as a basis for our expectations and actions, and such a construal provides

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us with no principles of inductive acceptance or belief concerning empirical hypotheses.²

Rules of inductive acceptance are then proposed to overcome this defect. These rules are supposed to specify a threshold-value upon which hypotheses are accepted relative to a body of evidence. Unfortunately, however, these rules generate a paradox---the lottery paradox which shows that on certain types of evidence, logically incompatible hypotheses would have to be adopted. Therefore, these rules are not tenable either.

Hempel maintains that all these difficulties may be overcome when the acceptance of a hypothesis is seen not as an inference but as an instance of cognitive decision making. According to Hempel's account, this idea was originally derived from the exchange between Rudner and Jeffrey in the early fifties.³ The idea here is that the acceptance of hypotheses is determined not only by the relevant evidences but also by the desirability or undesirability (epistemic utility) attached to the acceptance or rejection of that particular hypothesis. Along this line, Hempel himself has helped to develop the view that the acceptance of a hypothesis may properly be construed as an act of decision, though it is a kind of cognitive act which is distinct from an act of the practical kind. According to Hempel, it is a decision which involves admitting a hypothesis into the knowledge corpus and which

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has the goal of improvement and cumulation of scientific knowledge. The central question of cognitive (epistemic) decision, under this interpretation, is the choice of hypotheses which satisfy certain rational decision rules relative to certain cognitive goals. As we shall see, this idea is elaborately developed by Isaac Levi in his epistemic decision model which is one of the major concerns of our inquiry.

Briefly, this is our chief motivation for adopting the decision-theoretic framework. We hope that by adopting such an approach to the Duhem problem, a new perspective on the problem as well as a solution to it can be gained. As we have just mentioned, the decision-theoretic framework we are employing here is the epistemic decision model proposed by Levi. It is hoped that the Duhem problem can be solved in an interesting way via Levi's model.⁴

Admittedly, our task here should at best be seen as highly exploratory in nature. As a matter of fact, the decision-theoretic programme in general and Levi's model in particular, are budding programmes. It should not be seen as any fierce competitor to the traditional inference model. Whether the programme can be successfully developed into a genuine alternative is taken here as an open question. Our investigation, after all, can only be seen as a first step in examining its potential as well as weaknesses within the locus of the Duhem problem.

FOOTNOTES

1. There are other difficulties as well. For detail, see Hempel [1981, pp. 390-391]. Interestingly, Hempel's paper provides a good overview of the problems involved. Our following discussion in this chapter, therefore, follows it in major points.

2. Hempel, ibid, p. 392

3. cf. Rudner (1953), Jeffrey (1956).

4. There are certainly interesting questions to be raised about the epistemological properties of Levi's model. However, this is beyond our present concern.

CHAPTER II

DECISIONS AND SCIENTIFIC INQUIRY

Decisions play an important role in scientific inquiries. Perhaps this is a claim not many would readily accept without an argument. The lack of consensus on this issue nonetheless should cause no surprise---the claim itself is simply too equivocal and vague for one to make a clear judgement on it. In the first place, one would ask what is the meaning of decision referred to here? Second, even if we do have a clear sense of what decision means here, we still have to understand in what way decisions function in scientific practices. Obviously, these are the questions that should be answered in any decision-theoretic approach to scientific inquiry. In this chapter, our primary objective is to try to clarify the sense in which scientific inquiry is seen as decisions. This is done by demonstrating how decisions are operative on one level of scientific deliberation---the choice of basic statements. Though we believe that decisions are also operative on other levels of scientific deliberations, we think that by demonstrating how the choice of basic statements invokes decisions we are able to give support to the claim stated at the beginning of this chapter.¹ Interestingly enough, we shall find that even those who do not explicitly commit themselves to a decision-theoretic approach to science do somehow presuppose something close to our claim. Moreover,

the recognition of the fact that science is in a sense a kind of decision would help to render the relevance of decision theory to science a little more explicit. In this connection, we shall offer a brief review of Hempel's and Levi's view on this issue. Finally, we shall try to compare the structures of practical decision making and cognitive decision making so as to pinpoint the distinctive features of the latter type of decision making.

DECISIONS AND BASIC STATEMENTS

In the heyday of Logical Positivism there were great interest in the discussions of the problem concerning the "empirical basis" of scientific knowledge. Simply put, the problem of empirical basis is about the logical and epistemological nature of the foundations of empirical knowledge. Due to the logical positivists' predilections with the structure of scientific language, this problem is transformed into a problem about the nature and epistemological status of the kind of statements (sentences) which constitute the basis of empirical knowledge. These statements (sentences) are generally referred to as "protocol sentences," "basic statements," or "observation statements". These sets of statements, as their names probably suggest, are purported to represent our primary sensory experiences. Hence, they are supposed to be the

"foundation stones" of our knowledge of the external world. They are regarded as the bases upon which other statements are "erected". With the exception of Neurath, logical positivists in general see these statements as having absolute certainty and thus immune to revision. These statements, according to the logical positivists, constitute a "privileged class". In contrast, Popper much earlier on had pointed out that both the determination of basic statements and the selection of basic statements are indeed some kind of decisions on the part of the investigator. Since basic statements constitute an important part of scientific knowledge, to see how decisions are involved in their determination would undoubtedly lend credibility to the claim that decisions constitute an important part of scientific practices.

Wittgenstein perhaps was the one originally responsible for arousing so much interest in the discussion of basic statements in the earlier days of logical positivism. For Wittgenstein, the existence of elementary propositions is a logical consequence of his theory of meaning. To understand Wittgenstein's notion of elementary proposition, we have to understand the thesis of extensionality---the central thesis of his theory of meaning in his Tractatus period.

According to the thesis of extensionality, the truth of a proposition is determined completely by the truth of its components. Let us briefly elaborate on this. Wittgenstein

thinks that there are basically two kinds of propositions---simple propositions and complex propositions. Complex propositions are those propositions which contain simple propositions as well as other complex propositions as their components. Simple propositions, on the other hand, are those propositions which contains no other propositions, simple or otherwise, as their parts. To understand the meaning of a proposition, Wittgenstein maintains, one has to understand the meanings of its parts. Since complex propositions are composed of other propositions, their meanings naturally depend on the meanings of the latter. However, if the component proposition itself is composed of other components, then it too has to be analysed in terms of its components. But this cannot go on without stop, otherwise we cannot know the meaning of any proposition at all. As we are quite sure that we do know the meaning of at least propositions, then there must be some anchorage point where no further analysis is required. According to Wittgenstein, simple propositions are exactly the kind of propositions on which meanings of all other propositions depend. Due to this function, they may be regarded as the basic unit of meaning, or the ultimate meaning fixer. Thus, in order to understand a complex proposition, its meanings have to be analysed until the meaning of a simple proposition is reached. On the other hand, because Wittgenstein and many logical positivists think that truth condition is identical with meaning condition---the meaning

of a proposition is its truth condition, hence, what we have just said about the meanings of propositions applies equally well to the truth of propositions.

This theory of meaning and truth seems readily applicable to epistemology. As we shall see, the result of applying this theory to epistemology would yield something very close to a Humean analysis of empirical ideas---the meaning of a empirical idea is traceable to the impression from which it is derived. With regard to the justification of propositions, this same idea is operative as well. Basic statements, which are basically the epistemological counterparts of Wittgenstein's elementary propositions, are indeed the ultimate epistemological justifier of all other empirical statements. In this way, basic statements function in epistemology in nearly the same way that simple propositions function in semantics.

Incidentally, this is exactly what Schlick, Carnap and others intend as the function of basic statements. In fact, for Carnap and Schlick, a basic statement is in effect the epistemological interpretation of Wittgenstein's elementary proposition. However, unlike Wittgenstein's original notion, basic statements are supposed to represent empirical facts and do not purport to refer to metaphysical entities (like Wittgenstein's simple object).

According to Schlick, basic statements refer to raw, primitive sense data of experience---they are records of our direct experiences. Due to this nature, they are regarded as the most basic elements that constitute empirical knowledge. In addition, Schlick holds that they not only serve as an incorrigible foundation of knowledge, but also as a criterion of truth. [Schlick 1934, pp. 209-210].³ That Schlick holds such a view is not surprising because he thinks that the so-called problem of empirical basis is in no way different from the problem of the criterion of truth. If basic statements are interpreted as the certain and incorrigible basis of knowledge, they can also be taken as the criterion of truth of other empirical statements.

Though there were no significant disputes among logical positivists concerning the centrality of basic statements, not every one would agree on their epistemic status. Should they be regarded as incorrigible, immune to revision? Or, should they be treated on the same par with other empirical statements? Schlick, and Carnap at one time, think that basic statements should be given a privileged position in the sense that they are not open to revision. They believe that basic statements, unlike other statements, require no verifications. Against this, Neurath argues that no statement, whether basic or otherwise, should be seen as immune to revision; every statement should be subjected to verification. There is no privileged class of statements

that can be exempted from verification, criticism, revision and even rejection. [Neurath 1932] On the matter of revising scientific statements, Neurath's famous analogy of mending a floating vessel clearly implies that no statement in science should be treated as sacrosanct, and no statement should be viewed as immune to removal.

In the same vein, Quine nicely echoes this beautiful metaphor of Neurath in his "Two Dogmas." Though Quine and Neurath never explicitly mention the word "decision", it seems that the revision in question may implicitly demand some sense of decision, however vague, in situations like this. However, the main difference between Quine and Neurath concerning statement revision is this: while Quine regards simplicity as the only rational constraint on cognitive revision, Neurath allows no rational constraint except convenience. [Neurath 1935] This position immediately prompted Popper's criticism:

"we need a set of rules to limit the arbitrariness of "deleting" (or else "accepting") a protocol sentence. Neurath fails to give any such rules and thus unwittingly throws empiricism overboard....Every system becomes defensible if one is allowed (as everybody is, in Neurath's view) simply to "delete" a protocol sentence if it is inconvenient." [Popper 1968, p. 97]

Popper's remark here should be understood in conjunction with his belief that the revision of protocol sentences is in fact a decision. Indeed, perhaps more than anybody else, it was Popper who first clearly stated that the choice of basic statements is indeed a decision. He says:

"Basic statements are accepted as the result of a decision or agreement; and to that extent they are conventions. The decisions are reached in accordance with a procedure governed by rules. Of special importance among these is a rule which tells us that we shall not accept stray basic statements---i.e. logically disconnected ones---but that we should accept basic statements in the course of testing theories; of raising searching questions about these theories, to be answered by the acceptance of basic statements."

[Popper 1968, p.106]

"Coming to an agreement upon basic statements is, like other kinds of applications, to perform a purposeful action, guided by various theoretical considerations." [Ibid.]

What is expressed in this long quotation seems pretty self-explanatory. What merits attention here is the fact that decisions involved are by no means arbitrary. Though Popper does not specify what kinds of theoretical

consideration are involved, the fact that theoretical considerations are supposed to be invoked suffices to show that the decisions are deliberate and non-arbitrary.

Let us elaborate on Popper's suggestions. From what is asserted by Popper, there seem to be two ways in which basic statements are said to be the results of decision. First, decisions have to be made in determining what kind of sentences are to be regarded as basic statements. In other words, decisions have to be made in determining what logical structure basic statements would have. Logical positivists in general require basic statements to have sentence structures very similar to those of sense data sentences. The latter usually has the form of this kind: "Here, Now, perception-terms". Furthermore, they also want basic statements to be translatable into statements of physical language.

Neurath gives a more elaborate characterization of the structure of basic statements. He refers to basic statements as "protocol sentences" and thinks that they should have a complete form like the following:

"Otto's protocol at 3:17 o'clock: [At 3:16 o'clock Otto said to himself: (at 3:15 o'clock there was a table in the room perceived by Otto.)]" [Neurath 1932]

For Neurath, protocol sentences are indeed a special type of factual sentences which essentially contain personal nouns and perception terms. He remarks:

"Protocol sentences are factual sentences of the same form as the others, except that, in them, a personal noun always occurs several times in a specific association with other terms." [Ibid.]

Presumably, the specific way in which protocol sentences are constructed is supposed to serve some purposes. First, protocol sentences so constructed are capable of accommodating sentences which consist of reality-terms as well as hallucination-terms. This feature invariably gives them considerable flexibility in expressing immediate experiences. Second, protocol sentences are non-truth-functional---the truth of a protocol sentence is not determined by the truth of its component parts. Again, this makes protocol sentences capable of containing hallucination-sentences (sentences containing hallucination-terms) while remaining true. Though this gives considerable stability to the truth of protocol sentences, it should not be interpreted as giving protocol sentences incorrigibility, like Schlick and Carnap do to basic statements. In contrast, Neurath sees them as disposable as any other sentences. He emphatically says:

"Every law and every sentence of unified-science or of one of its sub-sciences is subject to...change. And the same holds for protocol sentences...."

"The fate of being discarded may befall even a protocol sentence. No sentence enjoys the noli me tangere which Carnap ordains for protocol sentences." [Ibid.]

The fact that protocol sentences are special sentences does not by itself make them less vulnerable than other sentences. Indeed, the occurrence of personal nouns and perception-terms in protocol sentences only serves the purpose of representing the "here, now" experience of a person, but it by no means guarantees their indispensability.

The point of the above discussion is not so much about the epistemic status of protocol sentences as about the fact that the determination of their forms indeed involves decision. Why does Neurath want protocol sentences to have the form he so intends? He certainly cannot rely on logic and experience alone, because they simply cannot tell him how to decide. Indeed, without invoking some sense of decision in this context, Neurath's move is hardly explicable. In other words, that protocol sentences have the form that Neurath intends is nonetheless the result of a

decision relative to certain theoretical considerations.

Surely it is Popper who explicitly acknowledges this decisional dimension of science. He clearly states that the determination of basic statements is a result of a decision. Furthermore, he refers to any singular statement which a person in an appropriate situation and using proper techniques can decide whether it is "acceptable" as a basic statement. [Popper 1934, s.34] Of course, the choice of singular statements as basic statements is not entirely arbitrary. According to Popper, singular statements are supposed to designate material objects, in this sense, they are not completely verifiable. This is because a sentence describing material objects can entail infinite sense data sentences but it itself is not entailed by the latter. So questions arise as to how much sense data is needed to decide whether a singular sentence is regarded as acceptable. Here we clearly have a decision problem. Meanwhile, the answer to this question, I suppose, largely depends on context. And decisions like this may vary from context to context. It also depends on how important the basic statement is with respect to the particular cognitive task in question.

In "Testability and Meaning", Carnap also holds a similar position. [p.134-135]. According to Carnap, a simple basic statement like "My pen is on the table" could very well entail an infinite number of sense data sentences.

It is both impossible and unnecessary to verify each and every member of the sense data sentences. Normally we take a finite number of verifications and accept the statement. Why is this so? The answer, again, lies in the pragmatic consideration of the situation. Unless we have good reason to do otherwise, in most cases, the above decision is always an acceptable one. Once again, this illustrates that decisions are the non-negligible facets of scientific deliberation.

We have discussed only one type of decision in determining basic statements. There is yet another type, also closely related to basic statements. This has to do with determining what the acceptable basic statements are. To achieve certain cognitive tasks, like testing a hypothesis, basic statements have to be selected. Being a basic statement itself does not automatically make it eligible for the task. Whether a basic statement is acceptable has to be determined relative to the specific context of inquiry. More specifically, it has to satisfy certain cognitive requirements. As Popper has pointed out before, since basic statements are accepted in testing theories or in helping us to raise searching questions about these theories, they certainly have to fulfil certain requirements in order to do the task. In view of this, Popper advises us not to accept "stray"---logically disconnected---basic statements in testing theories.

here is to show that the selection undoubtedly involves
 decisions.⁴

DECISIONS ON OTHER LEVELS OF SCIENTIFIC PRACTICE

In addition to those suggested by Popper, Lakatos identifies three more kinds of decisions within science. They are: decisions concerning the specification of rejection rules for probabilistic theory given statistically interpreted evidence; decisions of determining which part of the theory has to be removed when the theory faces a refutation; and finally decisions concerning whether "syntactically metaphysical" theories are to be rejected. [Lakatos 1970, p. 109-112] Incidentally, the fourth decision mentioned by Lakatos is exactly the version of Duhem problem that has attracted so much attention.

Lakatos not merely recognizes the existence of these various types of decisions in science, he also claims that in his methodology, decision occupies a central position. He says:

"decisions play a crucial role in [methodological falsificationism]." [Ibid. p. 112]

In a similar vein, Hempel also recognizes the decision dimension of science. He makes the following observation:

dimension of science. He makes the following observation:

"Science offers various examples [when] a conflict between a highly-confirmed theory and an occasional recalcitrant experiential sentence may well be resolved by revoking the latter rather than by sacrificing the former." [Hempel 1962, p. 621]

In cases alluded to by both Lakatos and Hempel, it is quite obvious that logic and experience can offer little help in guiding the investigator as to how to act. But in some cases, scientists do succeed in making reasonable moves. For example, they may succeed in rejecting certain portion of their theories and find they have good reasons for doing so. If this were the case, then obviously something more than logic and experience would be operative here. That "something more" perhaps could best be explicated via the concept of decision. As a matter of fact, referring back to Hempel's case, one of the reasons that we accept the highly-confirmed hypothesis rather than the recalcitrant experiential sentence is presumably that we value the former far more than the latter. We prefer the well-confirmed theory to the evidence sentence because the theory is supposed to possess certain desirable properties that the evidence presumably lacks. To express this

decision-theoretically, this is the same as saying that the theory has more epistemic utilities than the evidence has. Indeed, without presupposing something like epistemic utility, it seems highly inexplicable why an epistemic move like the one mentioned by Hempel could have taken place, or even be regarded as legitimate! Once again, our point here is to show that some sense of decision has to be assumed in understanding these kinds of cognitive deliberation.

The above discussion, nevertheless, should not be taken as a kind of justification of the decision-theoretic approach to science. Even if we admit that decisions, in whatever sense interpreted, do exist in science, this by itself does not suffice to justify the application of decision theory to the cognitive domain. Our justification, however, depends on a different argument. Decision theory is adopted here essentially because it can offer us a more systematical way to represent some salient features of cognitive decision making. By exploiting the concept of decision and other related notions, we hope a more adequate normative account of cognitive decision making can be forthcoming.

Though the adoption of decision theory to cognitive domain is based primarily on theoretical considerations, it is also partially based on a close similarity between practical decision making and cognitive or epistemic decision making. As a matter of fact, the construction of

decisions. Levi explains this point in the following way:

"...in the case of practical deliberation, the attainment of ends involves the selection of one option from among alternatives. And as in much practical deliberation, there is no guarantee that the option chosen will not fail. Thus, in certain respects, justifying reaching a conclusion via nondeductive inference is comparable to making a practical decision under conditions of uncertainty or risk. If this is so, then the general criteria for rational decision-making might be operative both in practical deliberation and in scientific inference." [GWT, p. 20]

In view of this consideration, the justification of the application of decision theory to scientific domain seems also depend on its empirical relevance. At this point, perhaps a brief explanation of the relationship between empirical fact and normative model is in order.

To provide a normative model of epistemic decision making is not simply to provide a logical model. Relevance is a key consideration here. Obviously the model should be a model of "something real". It is a model for a specific domain, therefore, its applicability is judged relative to that domain. Clearly, empirical evidence from that domain

domain, therefore, its applicability is judged relative to that domain. Clearly, empirical evidence from that domain affects, though not determines, how the model should be constructed. For example, in constructing an ethical theory, any model which is constructed in violation of some well-established evidence concerning human moral behaviours surely would significantly weaken, if not destroy, the plausibility of the theory. The same holds for models of epistemic decision making as well. We certainly do not want a theory which is only coherent and logically elegant, but has no application whatsoever to real situations. We surely want a model which can "guide" us in real science---normative model of real science. In this regard, we must take the empirical relevance of the model seriously. To speak metaphorically, a normative theory should have "one foot on the empirical domain and the other on the normative domain." After all, a good normative theory should harness resources from both domain in a wise way. In the next section, we are going to examine how practical decision making and cognitive decision making share some structural similarities.

PRACTICAL DECISIONS AND COGNITIVE DECISIONS

In order to argue for the relevance of decision theory in the cognitive domain, it is appropriate here to review

the structural similarity between practical decision making and cognitive decision making. Roughly, the common features between practical decision making and cognitive decision making are as follows:

- (1) There is a decision maker X who has certain specific objectives in mind and he is confronted with a problem of choosing a set of alternatives in achieving those goals.
- (2) There is a set of conditions representing the objective states of affairs, which, given a particular choice, would uniquely determine the outcomes.
- (3) There is a set of outcomes which is contingent on both the alternatives taken and the relevant states of affairs.
- (4) X has different preferences among these outcomes.

We can systematize the above factors by means of a decision matrix. Let S be the states of affairs, O be the outcomes and A be the set of alternatives. For simplicity sake, suppose we have only two states of affairs and two alternatives. The decision problem is then represented by a 2×2 matrix:

	S_1	S_2
A_1	O_{11}	O_{12}
A_2	O_{21}	O_{22}

FIG. A —
A 2×2
DECISION MATRIX

With regard to this matrix, X may be uncertain as to whether the states will obtain. In this situation, the decision is referred to as decision under uncertainty. Probability values are then assigned to the states of affairs indicating the uncertainty. The problem X now faces is to choose an alternative which will bring about the most desirable outcome. In this simplified picture of decision making, X's choice is to be made on basis of the following considerations:

- (1) The desirability of the expected outcomes of his choice.
- (2) The likelihood of the states of affairs.

On basis of the above characterization of a decision problem, we can generalize it to a $m \times n$ matrix to cover more general cases. Let us call the set of states of affairs "event-states set". It is an exhaustive set of event-states $S = \{S_j\}$, whose members are mutually exclusive of each other. A is a set of alternatives: $A = \{A_i\}$, where each A_i is an act or a strategy. Indeed, an act (or a course of action) can be regarded as a function which assigns a consequence to each state of the world. [Fishburn 1964, p. 50] Finally, we have a set of outcomes (consequences) $O = \{O_{ij}\}$, where each O_{ij} is the combined result of the fact that if A_i is implemented and S_j is the state that obtains. We can represent the $m \times n$ matrix as follows:

	S_1	S_2	\dots	S_n
A_1	O_{11}	O_{12}	\dots	O_{1n}
A_2	O_{21}	O_{22}	\dots	O_{2n}
\vdots	\vdots	\vdots	\vdots	\vdots
A_m	O_{m1}	O_{m2}	\dots	O_{mn}

FIG. B. — A $m \times n$ DECISION MATRIX

Let us use an example originally suggested by Savage [1972, pp. 14-15] to illustrate the ideas here. Suppose X's objective here is to make an omelet. He has already broken five good eggs into a bowl. He is thinking of whether to break a sixth egg into the bowl. The sixth egg then constitutes the world of X. Accordingly, there are two states of the world:⁵ the egg is a good one or the egg is rotten. There are three courses of action that X may take in this situation: (1) break the egg into the bowl which already contained 5 good eggs, (2) break it into a saucer for inspection first, and (3) throw it away.

Obviously not every outcome is equally desirable for X. In standard decision theory, a utility function is used to represent the differences in the desirability of these various outcomes. Simply put, a utility function is a

function which assigns numbers to outcomes and which satisfies certain constraints on preferences over outcomes.⁶ That is, $u(O_{ij})$ is a number representing the utility of outcome O_{ij} . The expected utility of each strategy is calculated in terms of the sum total of the expected utilities of the outcomes the implementation of that strategy produced. This Bayesian idea is expressed by the following formula:

$$eu(A_i) = \sum_{j=1}^n p(S_j) u(O_{ij})$$

Here $eu(A_i)$ is the expected utility of alternative A_i , $p(S_j)$ is the probability of event state S_j , and $u(O_{ij})$ is the utility of outcome O_{ij} , when the alternative A_i is chosen and the event state S_j obtains. According to the Bayesian rule of maximizing expected utilities, the alternative which scores the maximal expected utility value is the optimal option. This, in a nutshell, is the basic structure of practical decision making and the related decision rule of maximizing expected utility.

COGNITIVE DECISION MAKING

The structure of practical decision making presumably gives us some insights into the structure of cognitive decision making. Hempel, among others, clearly recognizes

this. For him, a cognitive decision is something like this:

"...a scientist has at his disposal the set of all sentences accepted at the time which we may assume to be expressed in the form of one complicated sentence e ; that he has invented or has been presented with a set of n hypotheses $h_1, h_2, h_3, \dots, h_n$, which, on e , are pairwise incompatible while jointly exhausting all possibilities...and that he has to choose one from the following $n+1$ courses of action: to accept h_1 and add it to e ;...; to accept h_n and add it to e ; to accept none of the n hypotheses and thus leave e unchanged. The problem is to construct a rule that will determine which choice it is rational to make... which, if any, of the proposed hypotheses is to be 'accepted' and thus to be added to the corpus of scientific knowledge." [Hempel 1965, p. 75]

This passage itself is pretty much self-explanatory. The cognitive decision problem is conceived here as the determination of what the optimal hypothesis relative to the set of hypotheses is, given the evidence.

In a similar vein, perhaps in a more specific way, Levi also tries to specify the cognitive decision problem as

follows:

"An investigator who is interested in making predictions or estimates, or in deriving generalizations from confirming instances and other evidence, will be construed as deciding what conclusions to reach via induction from given evidence. His decision will be taken to be a "cognitive option" analogous to "practical options," although no cognitive option will be understood to be a decision to act relative to a practical objective. A system of "states of nature" will be constructed and outcomes of the cognitive options for various states of nature will be specified. An attempt will be made to construct an "epistemic utility function" for these outcomes..." [GWT, p. 54]

From Hempel's and Levi's characterizations, it is pretty clear that the objective of a cognitive decision theory is the construction of rational rules whereby scientists could determine optimal cognitive options. As we have already pointed out, cognitive decisions as well as practical decisions both invoke a host of contextual factors. In the practical case, we have the objectives of the agent, the relevant states of nature, the set of preferences defined over the outcomes. Analogously, similar factors are also

involved in the cognitive situation. In the next section, we shall look more closely into these cognitive factors embedded in a cognitive decision context.

COGNITIVE CONTEXTUAL FACTORS

In our previous discussions, we have been vaguely indicating that cognitive decisions, like practical decisions, do consist of similar contextual factors relevant to the decisions in question. Now let us be more specific on this issue.

We take the view that there are many types of cognitive decisions. For example, making prediction, estimation, simple generalizations, accepting potential explanations---are notable cognitive decisions. Moreover, pondering whether to run an experiment (to test a hypothesis) may also legitimately be regarded as making a cognitive decision. Whether all these kinds of cognitive decisions can be nicely fitted into a standard decision format (as presented in the previous section) is regarded here as an open question. Our task here is to show one type of decision which could be properly structured into the standard decision format. The type of decision we have in mind here is that proposed by Levi in his GWT. By means of Levi's cognitive decision model, we shall begin to appreciate the structural similarity between practical

decisions and their cognitive counterparts.

The decision problem which concerns Levi is about an agent whose interest is to choose a true and informative answer from among a set of potential answers relative to a given question. Cognitive decisions of this sort are referred to by Levi as "efforts to replace agnosticism by true belief." [GWT, p. 58] In other words, truth and information are the two epistemic goals of the agent. Whether an epistemic option is optimal depends on whether it can best realize the epistemic goals of the agent. In contrast to practical decisions, the options, outcomes and utilities involved in a cognitive context are not the same as those in a practical context, it is proper then to explicate their meanings here. To facilitate understanding, I suppose these notions can best be explicated by means of an example.⁷ Suppose A, an investigator, wants to make a forecast of a horse race which has three horses competing. Let the horses be X, Y, and Z. The cognitive options for A are the following: (i) X will win, (ii) Y will win, (iii) Z will win, (iv) X or Y will win, (v) X or Z will win, (vi) Y or Z will win, (vii) X or Y or Z will win and (viii) X and Y and Z will win. Though eight options are available, they are not equally attractive for A relative to his goal of obtaining error-free information. Surely, each cognitive option, i.e. the acceptance of either one of the eight options, may offer different degrees of satisfaction of the

goals of A. In other words, each option may produce a different outcome. The cognitive outcomes referred to here are either the truth or the relief from agnosticism the acceptance of each option provides.

On the other hand, if truth and information are the two objectives to be accomplished, it also means that truth and information are the two features of the answers (outcomes) that are regarded as desirable by A. By the same token, we say that these outcomes possess various degrees of epistemic utilities representing their respective cognitive values. Cognitive decisions here are also understood as a version of decisions under uncertainty. That is, decisions are made in situations where the agent is uncertain of whether the states of nature will obtain. So, in addition to epistemic utilities we also need probabilities of the states of nature. Analogously, the same Bayesian principle of maximizing expected utility seems to operate here as well. Accordingly, the optimal cognitive option is the one which has the maximal expected utility. The expected epistemic utility of an option is computed by the following formula:

$$eu(A_i) = \sum_{j=1}^n p(S_j) u(O_{ij})$$

where $eu(A_i)$ is the expected epistemic utility of option A_i , $p(S_j)$ is the probability of the state S_j , and $u(O_{ij})$ is the epistemic utility of the cognitive outcome O_{ij} .

This, in sum, is the structure of epistemic decisions
and the related decision rule.

FOOTNOTES

1. This is not to say that the sense of decision involved here is the full-blown sense of decision in classical decision theory.

2. Wittgenstein uses "elementary proposition" and "simple proposition" interchangeably.

3. All references to Schlick and Neurath in this chapter are from Ayer [1959].

4. Some might argue that since basic statements are peripheral in scientific choices, decision theory is not needed here. Though it is true that no classical sense of decision is invoked here, it does not preclude the possibility that a more complete theory of decision might be developed in this area. For a close example, see Goosens' paper in Bogdan [1976] and Levi's paper [1977].

5. According to Savage, the world is the object about which the decision maker is concerned, and a state of the world is "a description of the world, leaving no relevant aspect undescribed." ([Savage, 1972, p. 93].

6. Others take a utility function to be a function defined over lotteries and which satisfies a system of formal constraints on preferences over lotteries. cf. Luce and Raiffa [1957, ch.2].

7. see Levi, GWT, pp. 37-38.

CHAPTER III

WHAT IS THE DUHEM PROBLEM?

Grünbaum, Laudan, Quinn, Hesse and others have succeeded in making considerable progress towards a better understanding of the complexity of the Duhem problem. Nevertheless, in the course of attacking and defending the Duhem thesis, different versions of the Duhem problem have been proposed. Though it is surely impossible to review all the versions proposed in the literature here, it is interesting to review one version that has been proposed by Adolf Grünbaum which incidentally has succeeded in arousing considerable interest in the debate relating to the Duhem problem.

In several recent articles closely related to the Duhem problem [Grünbaum 1960, 1966, 1974], Grünbaum tries to interpret the Duhem problem as consisting of the following claims:

1. The logic of disconfirmation (confirmation) of an empirical hypothesis H is such as to involve an entire network of interwoven hypotheses in which H is an integral part.

2. No single hypothesis H can be sufficiently isolated from the network of hypotheses for conclusive falsification (disconfirmation). [Grünbaum 1960, pp. 116-117]

According to Grünbaum, (2) itself entails the following claims:

(a) If the observational outcome O is entailed by the conjunction of H and a set of auxiliary hypotheses A , the failure of O to obtain entails, by modus tollens, that both H and A be false and not that H be false. That is, the falsification of H is not conclusive exactly because $\neg H$ is not deducible by logic alone, from the premise $\{[(H \& A) \rightarrow O] \cdot \neg O\}$.

(b) The actual observational outcome O' , which is incompatible with O , still keeps H intact, because it is in principle possible to construct a revised version A' of A such that the conjunction of A' with H will entail O' . [Ibid. p. 117]

From (b), we have a seemingly striking result. Grünbaum states it clearly as follows:

"...at the price of suitable compensatory modifications in the remainder of the theory, any one of its component hypotheses H may be retained in the face of seemingly contrary empirical findings as an explanans of these very findings. And this quasi a priori preservability of H is sanctioned by the far-reaching theoretical ambiguity and flexibility of the logical constraints imposed by observational evidence."

[Ibid.]

Based on this interpretation of the Duhem problem, Grünbaum puts forward an argument against the Duhem problem to the effect that either it is false or it is a non sequitur. Our problem is not one of assessing whether his argument is sound, but whether his representation of the Duhem problem is a fair one. To do this, of course, we have to refer to Duhem's own work.

IN DUHEM'S OWN WORDS

In his celebrated work---The Aim and Structure of Physical Theory---Duhem tries to take issue with a view of science generally held by scientists of his time. According to this view, scientific hypotheses can generally be falsified by setting up "crucial experiments." Duhem does not think that this view is a correct representation of the logic of scientific testing. To refute such a view, Duhem explicitly states what he regards as some general claims pertaining to scientific testing:¹

- (1) No individual scientific statement or hypothesis by itself has any observational consequence.
- (2) No individual scientific statement or hypothesis can be conclusively verified.
- (3) No individual scientific statement or

hypothesis can be conclusively verified by any outcome of crucial experiments.

(4) No individual scientific statement or hypothesis can be conclusively verified by induction from experience. [Duhem 1962, pp.144-164]

Let us see what can be made out of these claims. It seems that Duhem has not made it very clear what he means by saying that individual statements by themselves entail no observational consequences. For the sake of argument, however, let us assume the truth of (1). Given the truth of (1), it is quite obvious that statements which entail no observational consequences certainly are not testable in any meaningful sense of the word. Scientific testing, in one essential sense, is the testing of whether the predicted observational consequences of hypotheses or theories obtain. Thus, scientific testing is surely impossible if the hypotheses under test have no observational outcome.

Though theoretical statements are not singly testable, it does not mean that they are not testable when they are conjoined with other scientific statements. Duhem maintains that theoretical statements are testable when they are conjoined with other statements. That is, they are testable only as a group and never in isolation. Indeed, it is exactly due to this nature of collective testing that

scientific testing is inherently inconclusive---there is no possibility that one can ever have a conclusive verification (confirmation) or conclusive falsification (disconfirmation). Duhem explains:

"...if the predicted phenomenon is not produced, not only is the proposition questioned at fault, but also is the whole theoretical scaffolding used by the physicist. The only thing the experiment teaches us is that among the propositions used to predict the phenomenon and to establish whether it would be produced, there is at least one error; but where this error lies is just what it does not tell us." [Ibid., p.185]

At other places, Duhem reiterates the same point:

"...when the experiment is in disagreement with his predictions, what [the scientist] learns is that at least one of the hypotheses constituting this group is unacceptable and ought to be modified; but the experiment does not designate which one should be changed." [Ibid. p. 187]

It is clear from these passages that Duhem subscribes to a holistic view of scientific testing. According to this view, the result of any given test is to be distributed over

the whole theory complex and not to be concentrated on any one specific component of it. In other words, the "blame" or "credit" resulted from the test is spreaded over the entire theoretical edifice.. And it is exactly because of this that one can never get a conclusive verification or falsification.

With regard to (3), and (4), presumably similar arguments can be constructed along the line just discussed. We shall not discuss them here.²

Let us briefly summarize what we have done so far. If our exposition of Duhem's view is correct, then it is not difficult to see why Grunbaum's version of the Duhem problem is a mistaken one. Obviously, Grunbaum has ascribed to Duhem a version which is much stronger than the one which Duhem himself would allow. Grunbaum's version, which states that any hypothesis can be saved by simply adding new auxiliary hypotheses to the explanans, is definitely not what the version Duhem has in mind. Indeed, Duhem's claim is weaker. It states only that falsification is necessarily inconclusive. It does not state that any hypothesis can be saved by constructing additional auxiliary hypotheses. Incidentally, it is Quine who does hold a view like this: We shall come to that in the next section.

QUINE'S VERSION OF THE DUHEM PROBLEM

Quine's idea in this regard is basically expressed in his influential essay "The Two Dogmas of Empiricism." Indeed, his position is largely an extension of Duhem's original ideas with the following exception. Duhem's view seems to be more methodological in nature whereas Quine's position is more of an epistemological and semantical nature. Quine in effect holds two views which are of special relevance to our discussion. The first view states that the unit of significance (meaning) is not individual statements but the whole language. Let us refer to this view as (Q-A) claim. (Q-A), as it were, resembles closely the Duhemian idea that individual statements cannot be tested in isolation but only as a group. Indeed, (Q-A) could be seen as a semantical counterpart of the Duhemian methodological claim. Quine explains this holistic semantical view as follows:

"...reevaluation of some statements entails reevaluation of others, because of their logical interconnections---the logical laws being in turn simply certain further statements of the system, certain further elements of the field. Having reevaluated one statement we must reevaluate some others, which may be statements logically

connected with the first or may be statements of logical connections themselves." [Quine 1953. p. 42]

This is presumably why Quine says, in another context, that the unit of empirical significance is the whole of science. Incidentally, this semantical view of Quine comes very close to a kind of semantical holism which asserts that the meanings of theoretical and observational terms are intimately tied together so much so that it is not very meaningful to differentiate one from the other. In other words, scientific terms, theoretical as well as observational, are semantically global---their meanings are interrelated in a global manner. Nevertheless, this is perhaps one of the reasons why Quine does not think that there is a clear-cut distinction between theoretical statements and empirical statements. For Quine, knowledge is not like a two-layered structure envisaged by the logical positivists. Instead, he sees it as a network of statements (empirical and theoretical) interrelated with each other:

"...the totality of our so-called knowledge or beliefs,...., is a man-made fabric which impinges on experiences only along the edges...."[Ibid. p.42]

Since knowledge is seen as a network the strands of which are statements of various sorts, the "knots" of the network presumably are places where our "web of beliefs" meets experience. Because of its network-like structure, our knowledge of the world is then regarded as coming into contact with the world as a whole. This is exactly how Quine envisions the relationship between knowledge and the world. He says:

"our statements about the external world face the tribunal of sense experience not individually but only as a corporate body" [Ibid. p. 41]

This statement incidentally represents the second Duhemian view of Quine. Let us call it (Q-B) claim. Contrasting with (Q-A), this claim is thoroughly epistemological in nature. Moreover, it also entails an epistemological conventionalism which is typically Quinean. According to the latter view, the logic of belief change is something like the following:

"A conflict with experience at the periphery occasions readjustments in the interior of the field. Truth values have to be redistributed over some of our statements...But the total field is so underdetermined by its boundary conditions, experience, that there is much latitude of choice as to what statements to reevaluate in the light

of any single contrary experience. No particular experiences are linked with any particular statements in the interior of the field, except indirectly through considerations of equilibrium affecting the field as a whole." [Ibid]

Incidentally, such a view of belief change is epitomized by Quine's often quoted radical conventionalist slogan: Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system-(RC).

Indeed, as we have pointed out before, it is this statement of Quine which is largely responsible for the position which Grünbaum has attributed to Duhem. Moreover, it is the radical conventionalism derived from this view that has generated so much debate, in the literature.³

Referring back to Quine's claim that any statement can be held true regardless of the observational outcome, "any statement" here means all statements within the knowledge corpus---theoretical and observational statements and even logical laws. He says,

"Even a statement very close to the periphery can be held true in face of recalcitrant experience by pleading hallucination or by amending certain statements of the kind called logical laws... no statement is immune to revision." [Ibid.

emphasis added.]

In his original formulation of (RC), Quine uses the word "true" instead of "well-confirmed" or "highly probable" to characterize the statements within the corpus. This is perhaps one of the reasons that (RC) appears to be rather radical in nature. According to Mary Hesse, (RC) could be properly weakened to a more plausible position simply by replacing the notion of truth by that of confirmation. Furthermore, the kinds of statements referred to in (RC) could be narrowed down to include only the descriptive statements. Accordingly, the Hessean version of (RC) may be formulated as follows:

(RC'):. No descriptive statement can be individually falsified by evidence, whatever the evidence may be, since adjustments in the rest of the system is always possible. [Hesse 1976, p. 188]⁴

Let us sum up Quine's view presented in this section. If our discussion of Quine's view is a correct one, then it is clear that Quine's part of the Duhem problem is composed of nothing more than the following claims---(Q-A), (Q-B), (RC) and (RC').

In sum, it is clear that in discussing the Duhem problem, one has to distinguish Quine's view from that of Duhem's. As we have clearly shown, Quine's view is much more radical. It is the main purpose of our discussion, therefore, to focus only on the core of the Duhem problem---the thesis of the ambiguity of scientific testing.

FOOTNOTES

1. cf. Quinn, [1974, pp. 37-45.]
2. Duhem, [1974, pp. 191-193, 199]. For exposition see Quinn, ibid., pp. 43-45.
3. In a letter to Grünbaum, Quine admits that such a view is too extreme and he points out that in his Word and Object, he opts for a more modest form of holism. [Harding, 1976, p.132].
4. According to Hesse, (RC') nonetheless requires a "theory of correspondence" to "support itself, I suppose that the alleged theory would run something like this: Observational terms and statements in science have their meanings determined (at least partially) by the theory whose domain they describe." Also, cf. Hooker, (1978, p. 64.)

CHAPTER IV

WAYS TO COMBAT THE DUHEM PROBLEM

In this chapter we are going to offer a critical review of some of the major ways proposed in the literature to combat the Duhem problem. As expected, the solutions to the Duhem problem are largely dependent on how the problem is interpreted. Therefore, we shall introduce both the interpretations of and the solutions to the Duhem problem. Amazingly, however, there is a considerable convergence of views on the interpretation of the Duhem problem. As we shall see, many writers take the ambiguity thesis as their focus of attention.

POPPER'S INTERPRETATIONS AND SOLUTIONS

In his earlier writings---The Logic of Scientific Discovery---Popper discusses the Duhem problem within the context of falsification. Basically, he takes the ambiguity thesis as the core of the Duhem problem. With regard to falsification of scientific theories, Popper concedes that the Duhemian idea of holistic testing is obviously at work. In falsification, Popper maintains that

"...we falsify the whole system (the theory as well as the initial conditions) which was required

for the deduction of statement p , i.e., the falsified statement. Thus it cannot be asserted of any one statement of the system that it is, or is not, specifically upset by the falsification."

[Popper 1968, p. 76]

Needless to say, this is exactly one of Duhem's important insights concerning the logic of scientific testing. Though testing is holistic, however, Popper argues that not every part of the whole theoretical system is equally affected by falsification. He maintains, for example, that part of the system of which p is independent apparently would not be affected. As to the other parts where such independence does not exist, Popper agrees that one can hardly tell which is responsible for the falsification. He says:

"...it is often only the scientific instinct of the investigator (influences, of course, by the results of testing and retesting) that makes him guess which statements...he should regard as being in need of modification." [Ibid. p. 76, n. 2]

Popper's statement here seems to suggest that instinct, or rather, habit, is the determining factor, and that there is no rational rule whereby we can determine which part of the system requires amendment. Near the end of the same chapter, however, Popper seems to back off from this

statement and in effect proposes a "methodological rule" in this regard. He says:

"We may, in some cases, perhaps in consideration of the levels of universality, attribute the falsification to some definite hypothesis---for instance, to a newly introduced hypothesis. This may happen if a well-corroborated theory, and one which continues to be further corroborated, has been deductively explained by a new hypothesis of a higher level. The attempt will have to be made to test this new hypothesis by means of some of its consequences which have not yet been tested. If any of these are falsified, then we may well attribute the falsification to the new hypothesis alone. We then seek, in its stead, other high-level generalizations, but we shall not feel obliged to regard the old system, of lesser generality, as having been falsified." [Ibid. p. 77]

Popper's idea can be represented schematically as follows: Let H be the newly introduced hypothesis which can deductively explain T , T be a well-corroborated theory, $\neg p$ be the falsifying instance, and p be a logical consequence of $H \rightarrow T$. That is,

$$(H \rightarrow T) \rightarrow p$$

$$\frac{-p}{- (H \rightarrow T)}$$

Popper's recommendation here is: retain T and remove H. The moral is clear---well-corroborated theories should be preferable to less well-corroborated ones. To be sure, Popper here is only using an example to illustrate the importance of corroborativeness in theory acceptance (or rejection). It is not clear whether he would allow it as a general methodological rule. Elsewhere Popper seems to make some suggestions to that effect.

This can be seen in the context where Popper tries to argue against the conventionalist on matters concerning ad hoc hypotheses. Conventionalists claim that a theory can be saved in a falsification situation simply by devising appropriate ad hoc hypotheses. The force of falsification, the conventionalists maintain, could be absorbed by the ad hoc hypotheses. As a result, the theory can be saved.

Should the construction of ad hoc hypotheses satisfy any rational rules? The conventionalist answer is that the setting up of these hypotheses should be viewed as a matter of convention. This seems to imply that no rational rules are needed. Popper disagrees. He strongly maintains that the introduction of ad hoc hypotheses should be governed by rules. As a matter of fact, he proposed a rule to the

effect that the introduction of ad hoc hypotheses should increase and not decrease the degree of falsifiability or testability of the whole system:

"As regards auxiliary hypotheses we propose to lay down the rules that only those are acceptable whose introduction does not diminish the degree of falsifiability or testability of the system in question, but, on the contrary, increases it."

[Popper 1968, pp. 82-3]

According to Popper, this rule apparently has the following effect. If the rule is observed, the introduction of ad hoc hypotheses will only "save" the theory temporarily. The addition of ad hoc hypotheses will subsequently increase the degree of the falsifiability of the theory and hence increase the chance of the final refutation of the theory in subsequent tests. Therefore, from Popper's point of view, no theory can ultimately be saved by the implementation of ad hoc hypotheses if his requirement is satisfied. Suggestive though this rule may be, however, as an answer to the ambiguity problem, it is too loose to be useful.

With regard to the Duhemian holistic approach to scientific testing, on the other hand, Popper's attitude is at best ambivalent. Though he readily agrees with the idea of holistic approach, he also maintains the possibility of

piecemeal testing. Indeed, he thinks that piecemeal testing not only is possible but pragmatically recommendable:

"Criticism never starts from nothing, even though every one of its starting points may be challenged, one at a time, in the course of the debate. Yet though every one of our assumptions may be challenged, it is quite impracticable to challenge them at the same time. Thus all criticism must be piecemeal." [Ibid.]

What can be said of criticism can equally well be said of hypotheses testing. Popper's idea here clearly resembles Neurath's famous metaphor of fixing a floating vessel. The problem of Popper's present proposal is, however, like the previous rule, its vagueness. Until more details are worked out, such a proposal will have little bearing on the provision of solution to the Duhem problem.

Elsewhere Popper seems to suggest that falsification can be localized in situations where we do independence proofs of axiomatic systems. The idea is basically this. Suppose a model is constructed so that it satisfies all the axioms of the system except the one whose independence is to be shown. The model, as it were, serves as a counter-example for this one axiom and hence also as a counter-example of the theory (let the axiom represents

the theory).[Popper 1972 , p.239]

One obvious defect of this proposal is that, even if it works, it only has very limited applications. May be it can work on a system with an axiomatic basis. But one can be very skeptical as to whether there exists such a system in real science. This is why we say that Popper's suggestion has very limited applications. In light of this consideration, Popper's proposal may have little relevance to our present task. Presumably Popper himself also realizes this. That is why he admits that in a majority of cases (in scientific testing), "it is sheer guesswork which of its ingredients should be held responsible for any falsification." [Ibid.] Notwithstanding Popper's various suggestion, he finally seems to agree with the Duhemians that rational rules are not possible in this regard.

As a fallibilist, Popper always defends the idea that no theory should be protected from refutation. He openly endorses a rule to the effect that "the other rules of scientific procedure must be designed in such a way that they do not protect any statement against falsification." [Popper 1968 , p.54] But this still does not answer the question of localizing falsification. To my knowledge, Popper has never provided a direct answer to this question except by asserting that the progress of science implicitly demonstrates that refutation is in fact localizable. Popper's argument is expressed as follows:

"It seems to me quite clear that it is only through these temporary successes of our theories that we can be reasonably successful attributing out refutations to definite portions of the theoretical maze. (For we are reasonably successful in this---a fact which must remain inexplicable for one who adopts Duhem's and Quine's view on the matter.) An unbroken sequence of refuted theory would soon leave us bewildered and helpless: we should have no clue about the parts of each of these theories---or of our background knowledge---to which we might, tentatively, attribute the failure to that theory." [Popper

1972 , p.243-44]

Popper's argument here is that if refutation had not been localizable, the progress of science would not have been possible. But there is certainly progress in science, therefore refutation (or falsification) can be localizable. Obviously, even if this argument works, it only amounts to a "partial answer" to the question here, because our problem here is not whether refutation is localizable, but how it is localizable.

LAKATOS' APPROACH TO THE DUHEM PROBLEM¹

Like Popper, Lakatos also shares a keen interest in the Duhem problem. His understanding of the problem is very similar to Popper's. His approach to the problem, however, cannot be well understood without some familiarity with his concept of a scientific research programme. Therefore, we shall first present a brief review of Lakatos' concept and see how it relates to his solution of the Duhem problem.

A scientific research programme (SRP hereafter), according to Lakatos, is a system consisting of a series of theories and a body of methodological rules---negative heuristics and positive heuristics. Structurally speaking, Lakatos thinks that all SRPs can be seen as having a "hard core". The hard core of a SRP, as it were, represents the fundamental tenets and principles of the system. For example, in the Cartesian system, the mechanical conception of the universe as a huge clockwork and a system of vortices is considered to be its "hard core". The negative heuristics, on the other hand, are methodological rules to tell the researchers what paths of research to avoid. According to Lakatos, they forbid us "to direct the modus tollens at [the] hard core." [Lakatos' 1970 , p.133]

The negative heuristics also have the function of urging the investigator to devise "auxiliary hypotheses" which form a "protective belt" around the hard core. The protective belt, as it were, functions as a "sandbag" to absorb the

impact of falsification. Lakatos remarks,

"It is this protective belt of auxiliary hypotheses which has to bear the brunt of tests and get adjusted and readjusted, to defend the thus-hardened core." [Ibid.]

One classic example of how negative heuristics function to save the hard core of a SRP is Newton's gravitational theory, according to Lakatos. When it was first proposed, Newton's gravitational theory was confronted with a host of anomalies. But Newton was in no way to give up his theory. Instead, he tried to direct the impact of the anomalies away from the hard core by constructing auxiliary hypotheses. These hypotheses did in fact help to absorb the impact. Thus, Newton not only saved his theory from the anomalies, he also successfully transformed them into corroborating instances of his theory. The upshot is that not only the theory was saved but progress was made as well. This is a clear example of how negative heuristics helped Newton to redirect the modus tollens away from his hard core.

The setting up of auxiliary hypotheses here is very similar to Popper's idea of the construction of ad hoc hypotheses. In fact, Lakatos, following Popper, imposes a requirement on the provision of auxiliary hypotheses which runs very close to Popper's earlier suggestion. According

to Lakatos, the setting up of auxiliary hypotheses should increase the corroborated empirical content of the protective belt. [Ibid. p.134]

Unlike Popper, Lakatos' requirement demands the adding of auxiliary hypotheses only to increase the corroborated content of the protective belt, i.e., the auxiliary hypotheses, but not the system as a whole. But this difference is immaterial. As auxiliary hypotheses are logically related, no matter how loosely, to the theory and hence to the whole system, the increase of corroboration of them would ultimately distribute through the whole system. The point of increasing the corroboration of auxiliary hypotheses, to be sure, is to make auxiliary hypotheses themselves more testable. For the Popperians, this should be the case.

The positive heuristic of SRP, Lakatos maintains,

"[is consisted] of a partially articulated set of suggestions or hints on how to change, develop the "refutable variants" of the research-programme, how to modify, sophisticate the "irrefutable" protective belt." [Ibid. p. 135]

Roughly speaking, positive heuristics come very close to research guidelines or strategies. Using positive heuristics is like building a model of reality. It loosely

lays down instructions as to how to build the model. During the process of construction, actual data or counterexamples play no role in either speeding up or delaying the work of the construction. Indeed, in the construction process, not only one but a series of models are being constructed---better and better models are constructed one after the other. Such model building, Lakatos contends, is dictated by theoretical considerations more than anything else. Observations, incidentally, have no role to play here. Moreover, in the course of model building, positive heuristics also help us to anticipate "refutations" of any specific variant and are capable of providing ways digesting them. In sum, positive heuristics are guidelines to direct the possible ways that models are built, refined and developed.

So much for Lakatos' concept of SRP. Let us see how this concept bears on the Duhem problem. Using the machinery of SRP, there is a straightforward answer to the Duhem problem. That is, when observations come into conflict with the hard-core theory, negative heuristics should be used to redirect the modus tollens away from it. In other words, the recommendation here is to devise auxiliary hypotheses which serve as "protective belts" to "absorb" the falsifying experimental impact. The auxiliary hypotheses, according to the SRP approach, is by fiat supposed to carry the "blame" in such situations. Such a

response may be well justified when only the hard core is involved in the conflict. In cases, where theories, which are not the hard core of the SRP, are involved, it is not clear whether the same strategy will work as well. Incidentally, there is also another way of attacking the Duhem problem which involves a redefinition of the concept of falsification. In what follows we shall see how this approach works.

With Duhem, Lakatos finds that the naive falsificationist claim that individual theories can be falsified in isolation untenable. Consequently, he also regards their ways of solving the Duhem problem unacceptable.² The naive falsificationist way to solve the Duhem problem can be summarily expressed as follows: take the auxiliary hypotheses as background knowledge and treat them as unproblematic; then separate them from the hypothesis under test, subsequently an unambiguous falsification is obtained.

Given his concept of SRP, Lakatos thinks that falsification should not be taken in the manner envisaged by the naive falsificationist. For Lakatos, no theory can be falsified in the absence of an alternative theory. No matter how recalcitrant, a falsifying instance may be, Lakatos claims, a scientist should not abandon his theory until a better theory is available to him. This point is emphatically expressed as follows:

"...no experiment, experimental report, observation statement or well-corroborated low-level falsifying hypothesis alone can lead to falsification. There is no falsification before the emergence of a better theory." [Ibid. p. 119, original emphasis.]

A logical consequence of this statement is not difficult to draw:—

"...if falsification depends on the emergence of better theories...then falsification is not simply a relation between a theory and the empirical basis, but a multiple relation between competing theories, the original "empirical basis," and the empirical growth resulting from the competition."
[Ibid. p.120]

On basis of this understanding, Lakatos proposes a new definition of falsification. Accordingly, a theory T is falsified if and only if there is another theory T' which satisfies the following conditions:

(a) T' has excess empirical content over T, i.e., it predicts novel facts which are conceived by T as improbable.

(b) T' explains the previous success of T, i.e.,

all the unrefuted content of T is included (within the limits of observational error) in the content of T'.

(c) Some of the excess content of T' is corroborated.

(d) The evidence (or "empirical observation") which is the falsifying instance of T either is not a falsifying instance of T' or it is a corroborating instance of T'.³ [Ibid.p. 116]

Hence, the necessary and sufficient conditions for the falsification of a theory is the existence of another theory having the characteristics specified above. Needless to say, the alternative theory certainly is a better theory than the original one. Again, falsification is seen here not as a simple relation between evidence and theory as it is normally taken to be. Instead, falsification involves a triadic relation among evidence, the initial theory and its competing theory.

But has this new definition anything to do with the Duhem problem? The new definition of falsification, it seems, provides some sort of rational constraints on the falsification of theories. The problem Duhem originally raised implies that there are no such constraints---as long as we can set up ad hoc hypotheses to save the theory, nobody is certain whether a theory is being falsified.

Lakatos' definition, nevertheless, can be interpreted as providing conditions whereby we can determine whether a theory is falsified. That is, if we could avail ourselves of a better theory satisfying those conditions specified above, then we can say that a theory can be falsified unambiguously.

As we shall see, this response, like our approach, provides a similar solution to the Duhem problem. The search for a better theory within the Lakatosean framework bears a close resemblance to the choice of a better corpus within Levi's corpus revision model. The only difference, I suppose, is that the Lakatosean approach is basically a non-decision-theoretic one where utility is not an essential part.

There is one problem with Lakatos' answer. The conditions might work well with theories, but may seem too strong for hypotheses. Theories are far more complex than hypotheses, therefore even if they are confronted with anomalies, they probably have far more conceptual resources to absorb the anomalies than a normal hypothesis can have.

LAUDAN'S APPROACH TO THE DUHEM PROBLEM

Laudan's recent proposal on scientific progress incidentally has some interesting bearings on the Duhem

problem. Constructed mainly on the ideas of Popper, Lakatos and Kuhn, Laudan's model of scientific progress takes problem solving as the quintessence of scientific activity. In this section, we shall call his model a problem solving model.

There are two basic assumptions of Laudan's problem solving model:

1. The solved problem---empirical and conceptual---is taken as the basic unit in terms of which scientific progress is measured.

2. The objective of science is the maximization of (the scope of) solved empirical problems and the minimization of (the scope of) anomalous and conceptual problems. [Laudan 1977, p.66]

If science is a problem solving activity, then the construction of scientific theories is to solve problems. This idea is succinctly expressed by the following theses:

- (I) The crucial test of a theory is whether it provides satisfactory solutions to important problems.

- (II) The merits of theories are appraised not on bases of whether they are "true," "corroborated," "well-confirmed," et cetera, but whether they constitute adequate solutions to important problems.

Obviously, (I) and (II) can also be regarded as two criteria

of theory appraisal, though in a rather broad sense. One distinct aspect of Laudan's model, nevertheless, is the idea expressed by (II)---theories are not assessed in terms of their truth, corroboration or confirmation, but in terms of their problem solving capacity. For Laudan, the problem solving capacity or effectiveness of a theory is measured by the number and importance of the empirical problem the theory solves minus the number and importance of the anomalous and conceptual problems the theory generates. [Ibid., p.68]

Admittedly, Laudan's proposal is an interesting one. However, whether it will succeed as an alternative measure of scientific progress is not our concern here. What we have to know here is whether Laudan's concept of problem solving effectiveness has any direct relevance to our concern. This shall be examined in the final chapter of our essay. For the moment, let us see how Laudan's model is related to the Duhem problem.

Like many writers, Laudan construes the Duhem problem as the problem of ambiguity of scientific testing. He expresses the problem as follows:

"[In face of an anomaly], all we learn ...
is that we have gone astray somewhere, but
the logic of scientific inference is too
imprecise to allow us with certainty to pin the

blame on any particular component or components in the theoretical complex. It follows that we can never legitimately claim that any theory has ever been refuted." [Ibid., p.41]

Laudan readily agrees with Duhem that theories are not to be tested in isolation but only as a group. Moreover, within Laudan's model, we will have a Duhem problem, in the sense just presented, only when we face an anomalous problem. On the other hand, the ambiguity thesis is also transformed into the following form:

(A-1): If T (a theory complex) encounters an anomaly a, then a counts as an anomaly for each nonanalytic element t_1, t_2, \dots, t_n of T. [Ibid., p. 42]

Thus formulated, (A-1), according to Laudan, has the advantage of escaping the Duhemian charge in the following sense. The original Duhem problem is formulated in terms of assigning truth values to the components of a theory complex; but under this formulation, the relevant question is not the assignment of truth values but the attribution of problem solving effectiveness. Therefore, the difficulty of assigning truth values does not arise under this formulation. Indeed, he maintains that the advantage of the present formulation is gained from the fact that "there

is nothing in the structure of deductive logic which precludes the localization of properties such as problem-solving effectiveness." [Ibid., p.43]

But in what sense is (A-1) capable of avoiding the Duhemian charge? As a matter of fact, (A-1) cannot be literally interpreted as "avoiding" the Duhemian charge. Instead, (A-1) should more fairly be said to transform the charge into a lesser damaging form---an anomaly now becomes a special unsolved problem for T. Laudan makes the point clearly:

"When we say that a is an anomaly for a theory T, we are not saying that a falsifies T, ..., rather we are saying that a is the sort of problem which a theory such as T ought to be able to solve...but which it has failed as yet to solve." [Ibid. p. 43]

Having an unsolved anomalous problem, though not sufficient to bring down a theory, is surely enough to make one cast one's doubt on its problem solving effectiveness. How does Laudan respond to this? With the help of (A-1), Laudan may argue, the way the problem is expressed seems to suggest a way out. In fact, Laudan proposes an even-spreading strategy by using (A-1). The even-spreading strategy in effect states that instead of localizing blame or credit in one

place, we simply spread it evenly among the whole theoretical complex. [Ibid., p.43] In other words, an unsolved problem of a theory complex becomes an unsolved problem for each and every part of the complex.

Does Laudan's strategy work? There are at least two difficulties with Laudan's approach. First, the even-spreading strategy in some cases would generate a contradiction within his own model.⁴ Recall that (A-1) asserts that when T encounters an anomaly a, a counts as an anomaly for each non-analytical element of T. There is another (A-2), analogous to (A-1), which is proposed to handle confirmation. (A-2) in effect asserts that whenever T adequately solves an empirical problem b, b is counted as a solved problem for each nonanalytic element of T. [p.43] Now as theory testing is supposed to be holistic, it is conceivable that for any two theory complexes T and T', there is a shared element, say, t between them. Suppose in a falsification situation, an anomaly a becomes an unsolved problem for T, which in turn, according to the even-spreading strategy, becomes an unsolved problem for t. Suppose, on the other hand, in a confirmation context, T' solves the anomaly a, then by parity of reasoning, a becomes a solved problem for t. Clearly we have a contradiction here.

We can give an example for this. Suppose we have a theory complex which solves the problem of Brownian motion.

T consists of Kinetic molecular theory and some optical theory O, among other things. Suppose T solves the Brownian motion. This means that Brownian motion is a solved problem for O. Suppose a rival theory T', which contains phenomenological thermodynamics and the same optical theory O, fails to solve the problem, then the Brownian motion problem becomes an anomalous problem for O. Therefore, Brownian motion is both a solved and unsolved problem for O. [Brown 1981, p.144]

Even if this difficulty can be overcome, there is yet another difficulty with Laudan's suggestion. That is, Laudan does not really solve the problem but only postpones it. Though the occurrence of an anomaly is not sufficient to bring down a theory, there remains a problem as to how to handle the anomaly. The question of how to revise the theory complex in order to accommodate the anomaly has still to be answered. Even Laudan himself admits this:

"...because the anomaly exists, and because science seeks to minimize anomalies, there is still the cognitive pressure on the scientific community to attempt to solve the anomaly. Resolving that anomaly will require, presumably, the abandonment (though not by virtue of its falsification) of at least one of the theories that composed the complex that was unable to deal

with the anomaly." [Laudan 1977, p.44, emphasis added.]

On the other hand, though logic does not preclude the localization of properties such as problem solving effectiveness, neither does it enable one to localize them. In light of these considerations, the problem of rational theory choice⁵ still remains unsolved.

THE BAYESIAN APPROACH TO THE DUHEM PROBLEM.

In this section we are going to discuss two ways of tackling the Duhem problem which use the Bayesian theorem in an essential way. Both ways take the ambiguity thesis of the Duhem problem as their focus of concern. Dorling uses the Bayesian theorem together with a Bayesian Personalist reconstruction of a case in the history of science to generate a solution to the Duhem problem. Koertge, on the other hand, exploits the Bayesian theorem to the effect that a condition of disconfirmation is constructed and thus offers a partial solution to the Duhem problem.

(A) ---DORLING'S BAYESIAN PERSONALIST APPROACH

Jon Dorling, in a recent article, tries to provide an analysis which he hopes can offer a general solution to the Duhem problem. [Dorling 1979] The claim he attempts to

establish is this:

"...if a Bayesian personalist reconstruction is adopted then a natural and instructive resolution of the Duhem's problem is automatically available to us." [Ibid., p.178]

We shall first present Dorling's Bayesian reconstruction of the Duhem problem. Suppose we have two hypotheses H and K which jointly entail some consequence E . Suppose E does not obtain, that is, we have $\neg E$. Now, clearly $p(H \& K, \neg E) = 0$. The problem here is to evaluate $p(H, \neg E)$. This could be easily obtained if we could get the value of $p(\neg E, H \& \neg K)$. But unfortunately we do not know the value of $p(\neg E, H \& \neg K)$. What we do know here is $p(\neg E, H \& K) = 0$. Apart from this, we have no knowledge of any other likelihoods. However, if we have a subjective account of what we assume to be the most plausible rivals K' , K'' etc. to K , then $p(\neg E, H \& \neg K)$ can in principle be computed based upon a knowledge of $p(\neg E, H \& K')$, $p(\neg E, H \& K'')$ etc., which are scientifically accessible likelihoods. Let us assume that $p(\neg K) = p(K') + p(K'') + \dots$ (not continued indefinitely). Since K' etc. can in general be a whole class of related hypotheses, there is no difficulty in getting the result. On the other hand, in order to assign probability, we must be able to partition the rival hypotheses into classes about which we

can say something definite. And we must employ as much as possible subjective probability to make the results sufficiently definite. Following these steps, Dorling maintains, the answer to the Duhem problem is forthcoming.

Dorling uses an example in the mid-nineteenth century astronomy to illustrate how it works. Let E be Adam's mid-nineteenth century computational result of the secular acceleration of the moon. This disagrees with the observation result E'. Let T be a well established part of Newton's theory which is used in the computation. Let H be the crucial auxiliary hypothesis which states that the effects of tidal friction do not appreciably affect the lunar secular acceleration. Let the subjective probabilities of T and H be

$$(a) \ p(T)=0.9$$

$$(b) \ p(H)=0.6.$$

We also have

$$(c) \ p(E, T \& H)=1$$

$$(d) \ p(E', T \& H)=0$$

We want to find out the values of $p(T, E')$ and $p(H, E')$.

By Bayesian theorem,

$$(e) \quad p(T, E') = p(E', T) p(T) / p(E')$$

$$(f) \quad p(H, E') = p(E', H) p(H) / p(E').$$

Assume, for simplicity sake, that T is irrelevant to H . It means that $p(H \& T) = p(H) p(T)$. This is equivalent to assuming that $p(H, T) = p(H, -T)$.

We then have

$$(g) \quad p(E', T) = p(E', T \& H) p(H) + p(E', T \& -H) p(-H)$$

$$(h) \quad p(E', H) = p(E', T \& H) p(T) + p(E', -T \& H) p(-T)$$

$$(i) \quad p(E') = p(E', T) p(T) + p(E', -T) p(-T).$$

To solve the problem, we need to know the values of $p(E', T \& -H)$, $p(E', -T \& H)$ and $p(E', -T)$. Incidentally,

$$(j) \quad p(E', -T) = p(E', -T \& H) p(H) + p(E', -T \& -H) p(-H).$$

By analysing the historical case and then fixing subjective probabilities and using simple algebraic computations, we finally get the result.⁶ We shall not go into the calculation here (as this will be presented in Appendix A). The result of this Bayesian computation is:

$$p(T, E') = 0.8976$$

$$p(H, E') = 0.003.$$

This result surely gives us an unambiguous answer as to what to accept and reject.

(B) --- KOERTGE'S APPROACH TO THE DUHEM PROBLEM

Koertge recently attempts to provide a solution, using the Bayesian theorem, to a restricted version of the Duhem problem. Her version of the Duhem problem is: "If T and H imply E, under what conditions does -E disconfirm T?" (Let T be the hypothesis under test, H be the auxiliary hypothesis and E be the observation consequence.) [Koertge 1978, p.264] Koertge claims that the answer to this question can be obtained by using Bayes' theorem:

$$p(T, -E) = p(T)p(-E, T) / p(-E)$$

Switching terms, we have

$$p(T, -E) / p(T) = p(-E, T) / p(-E).$$

From this, we get

$$p(T, -E) < p(T) \text{ if and only if } p(-E, T) < p(-E)$$

What this identity means is that -E disconfirms T if and only if -E is less likely, given T than it is in the absence of T. According to Koertge, this could be regarded as the condition of the disconfirmation of T. [Ibid., p.264-5]

The first term on the right hand side of the identity, i.e., $p(-E, T)$, can be expanded using total probability as follows:

$$p(-E, T) = p(H)p(-E, T \& H) + p(-H)p(-E, T \& -H).$$

Since $T \& H$ entails E, therefore,

$$p(-E, T \& H) = 0.$$

So the disconfirming condition can be written as:

$$(R) : p(-H)p(-E, T \& -H) < p(-E)$$

What does R mean? Intuitively, R is satisfied when each of the terms on the left-hand side of the inequality is small. When $p(-H)$ is small, it means that the rival hypotheses have low probabilities and that H has a high probability. Koertge calls this the P factor. On the other hand, when $p(-E, T \& -H)$ is small, this implies that the alternatives to H, when combined with T, is very unlikely to predict -E. This is referred to as the L-factor. According to Koertge, the disconfirmation of T is greatest when both the P-factor and L-factor are low.

Koertge regards the above condition as providing a partial answer to the question of when a theory is disconfirmed. To obtain a complete answer, she argues, not only probability assignment but also desirability assessment (of the hypotheses) should be put into consideration. Koertge says,

"...scientific decision making involves not only probability appraisals but also assessments of the relative scientific desirability of various options." [Ibid., p. 265]

What Koertge means by desirability of a hypothesis here, is presumably equivalent to what we refer to as the epistemic utility of a hypothesis. This is quite evident when she

regards empirical content, explanatory power, simplicity, depth etc. as examples of scientific desirable features. According to Koertge, in making cognitive decisions, not only should epistemic utilities be required, practical utilities, like the cost of testing a hypothesis, should also be considered. [Ibid.]

(C)---CRITICAL COMMENTS ON DORLING'S AND KOERTGE'S PROPOSALS

The chief defect of Koertge's proposal is its vagueness. Despite mentioning the importance of both probability and utility considerations in cognitive decisions, she has very little to say on the notions of probability and utility, let alone the problem of how probability and utility are to be combined. Moreover, there is a complete lack of explication of the utility notions. As a result, such a lack of systematic treatment of these issues inevitably weakens the effectiveness of Koertge's approach.

Besides, even if we disregard the utility considerations and only limit ourselves to probability considerations, Koertge's solution is also far from satisfactory. As Koertge restricts her problem to a rather limited scope, it is not clear whether her solution would have a wider application. On the other hand, even if we confine ourselves to the specific question she raised, the

answer she offers to the disconfirmation problem could at best be seen as a partial one. Recall that disconfirmation of T is greatest when both the L -factor and the P -factor are low. However, this is only a special case. When we have either a low P -factor and a high L -factor or a high P -factor and a low L -factor, the net result may also be relatively low. And the disconfirmation condition is satisfied. When we have a low P -factor and a high L -factor, it means that the rival auxiliary hypotheses have low probabilities while the conjunction of these rival hypotheses with T predicts $\neg E$ with a high probability. Conversely, we might have cases where rival auxiliary hypotheses are highly probable and the conjunction of T with these hypotheses predicts $\neg E$ with a low probability. Insofar as the products of these two sets of values are less than that of $p(\neg E)$, the disconfirmation condition is satisfied. If this is the case, then there is a wide range of values which satisfies the condition. The acceptable range of values of both the P -factor and the L -factor can be considerably wide. For example, values of P -factor or L -factor even up to 0.9 might be acceptable so long as the product is less than $p(\neg E)$. This also means that the disconfirmation of T is easily achieved. But this is counterintuitive. Many, especially the Duhemians, would object to Koertge's condition simply because the condition does not sanction the exploration of conceptual resources in the set of auxiliary hypotheses to salvage the theory.

Dorling's suggestion, on the other hand, is based on a rather debatable assumption. That is, it assumes that historical cases are easily be reconstructible along the Bayesian lines and that probability assignment (in this particular case, Dorling assumes that point estimation is unproblematic) is unproblematic. Whether Dorling's approach can turn out to be another viable alternative seems to depend on how successfully his assumption is vindicated. For the moment, I shall leave this as an open question.

FOOTNOTES

1. Incidentally, there is a different way to tackle the Duhem problem in Lakatos' discussion on sophisticated methodological falsificationism, see Lakatos [1970, section c].

2. Lakatos' reason is that they offer no "suitable guide for the rational reconstruction of the history of science." [Ibid., p. 117]

3. The original Lakatos' version does not include (d). But this addition not only is compatible with Lakatos' idea, but also makes his concept more explicit.

4. I am indebted to Professor James Brown on this point and the ensuing example.

5. Though Laudan apparently fails to see the weaknesses of his approach, he does however recognize that the real challenge of the Duhem problem lies not in the localization problem, but rather in showing how to devise rational strategies for selecting a better complex. Our basic approach to the Duhem problem benefits from his insight.

6. Let us briefly describe how to assign subjective probabilities, according to Dorling, to hypotheses. Here we have to determine $p(E', -T \& H)$, $p(E', T \& -H)$ and $p(E', -T \& -H)$. Let us see how to assign values to $p(E', -T \& H)$ first. E' was a highly unexpected result. Since there was no rival theories to Newton's theory that could give the result, so $p(E', -T \& H)$ must be very small. So, a value of $1/1000$ is given. As to the values of $p(E', T \& -H)$ and $p(E', -T \& -H)$, they are related to the case where it is assumed that tidal friction can produce effects on the lunar secular acceleration of the right order of magnitude. A probability of $1/20$ is given to both on the consideration of the fact that this value "is needed to select 11 sec/cent/cent out of anything between 1 and 20 sec/cent/cent." [Dorling, 1979, p. 182]

CHAPTER V

CONTEXTUALISM AS THE
EPISTEMOLOGICAL BASIS OF EPISTEMIC DECISIONS

Traditional epistemological discussion of justification of beliefs is invariably global. That is, beliefs are justified relative to the total system of beliefs held at a given time. In this sense, Plato, Descartes, Empiricists, Duhem and Quine are the notable globalists. The kind of justification they seek will be referred to here as global justification. Other epistemologists seek a more modest position by restricting justification in a narrower context. According to this approach, a belief is justified not relative to the totality of beliefs, but only with respect to the context of a specific inquiry. Let us call this kind of justification "local justification". To locally justify a belief or a set of beliefs, according to the localist, one need only invoke a proper subset of the whole set of beliefs at a given time. Using the language of Levi, a belief is justified relative to the evidence available, the relevant answers, the relevant background information and the problem in question. Levi says:

"[in a local context]...justification of conclusions reached from given evidence ought to be made relative not only to that evidence but to

a characterization of, what is to count 'as a
'relevant answer' to the problem raised." [GWT,
p. 32]

In view of this understanding, the central problem of local
approach to justification, according to Levi,

"is the establishment of criteria for determining
which of the relevant answer to a given question,
on the evidence available, is the best." [GWT, p.
5]

The most conspicuous feature of the local approach is
the introduction of contextual factors---evidence, problems
and answers, background information etc. In GWT, Levi
tries to construct a model of local induction by utilizing
these contextual factors. As we shall go into fairly
detailed discussion of Levi's model, the point to note here
is that though Levi's model is originally designed to handle
the problem of nondeductive inference, it can be readily
applied to other areas as well. In fact, Levi himself later
on extends its use to a more general area (see EOK).
Insofar as our present inquiry is concerned, we think that
the local methodology can be applied to handle the problem
of corpus revision as well. Of course, this broadening of
application requires a broader epistemological support.

This brings us directly to the doctrine of contextualism which is explicitly proposed by Levi for this purpose. Nevertheless, if we see local justification as a specific methodological approach, then contextualism could be seen as the general epistemological position implicated by it. Let us then examine the claim of contextualism.

THE DOCTRINE OF CONTEXTUALISM

Simply put, the doctrine of contextualism states that scientific inquiry is a highly context-dependent activity. To understand scientific activity, one must understand the relevant contextual factors influencing it. Specifically, for example, the acceptance of scientific hypotheses or theories, according to the contextualist view, is largely determined by a host of contextual factors, some cognitive and some practical. Regarding the cognitive factors pertinent to a given scientific inquiry, the specific problem the investigator has, the body of evidence, the potential answers identified, the epistemic goals of the investigator and the research programme the investigator is committed to and so on are the relevant factors. [EOK, p. 65] How these factors interact so as to determine a given scientific deliberation is indeed the central question to be answered in any contextualist approach to scientific inquiry. For our purposes, we shall only try to answer

those questions that are directly relevant to our present inquiry.

To say that science is a context-dependent activity seems only to state a rather trivial fact. What is crucial here nevertheless is to understand the exact roles these factors play in affecting scientific acceptance. More importantly, we should examine whether the inclusion of contextual factors in science will jeopardize the objectivity of science. This presumably is the question we should answer if we want to defend contextualism.

Philosophers have long misunderstood the role contextual factors play in cognitive matters. They tend to regard the introduction of contextual factors in the cognitive domain as introducing arbitrary and subjective elements into science and thus hampering the objectivity of science. Incidentally, this widely held view has been under attack by some writers. Kuhn, in discussing scientific change, for example, contends that such a view is no longer tenable. Interestingly, he also maintains, though implicitly, a version of contextualism. Specifically, he argues that scientific change is a highly context-dependent matter. But such context-dependency of science nonetheless does not necessitate the kind of arbitrariness that many would tend to think. Moreover, it does not entail that it is beyond critical control as well. Furthermore, Kuhn underscores the fact that in scientific

change, the problem is not whether there are good reasons for the change, but how scientists use these reasons to produce the change. According to Kuhn, two scientists working within the same research programme and sharing the same system of (epistemic) values may with very good reasons differ in how they actually utilize these values to make decisions. He says:

"...in many concrete situations, different values, though all constitutive of good reasons, dictate different conclusions, different choices. In such cases of value-conflict (e.g. one theory is simpler but the other is more accurate) the relative weight placed on different values by different individuals can play a decisive role in individual choice. More important, though scientists share these values and must continue to do so if science is to survive, they do not all apply them in the same way. Simplicity, scope, fruitfulness, and even accuracy can be judged quite differently...by different people." [Kuhn 1968, p. 262]

Kuhn's observation here seems reasonable enough. However, in addition to his observation, I would add the following. When two investigators have different bodies of evidence, it is quite natural to expect that they will arrive at

different conclusions. In some cases, however, two investigators sharing the same body of evidence might disagree on the weight of the evidence on the hypothesis under test. Moreover, even if they have no quarrel on this issue, they might have good reason to disagree on the criteria of assigning weights to the evidence. These factors, together with those mentioned by Kuhn, suffice to convince us of the relevance of contextual factors in scientific inquiry. Though our discussion has not shown how the contextual factors affect cognitive decisions, it also has not shown that contextual factors are beyond critical control. However, no argument has so far been established to the effect that the introduction of contextual factors are in principle beyond rational control. And we have reason to suspect that such an argument is forthcoming. Furthermore, due to the communal aspect of scientific inquiry, the values committed by scientists are normally subjected to criticisms and revisions. Hence, the fact that different values are involved in scientific inquiry does not imply that the commitments of scientists are dogmatic and not open to criticisms. In this context, Levi offers the same observation regarding the revision of a knowledge corpus:

"...taking context seriously in appraising expansions and contractions [of corpus] does not imply that revisions of these kinds are immune

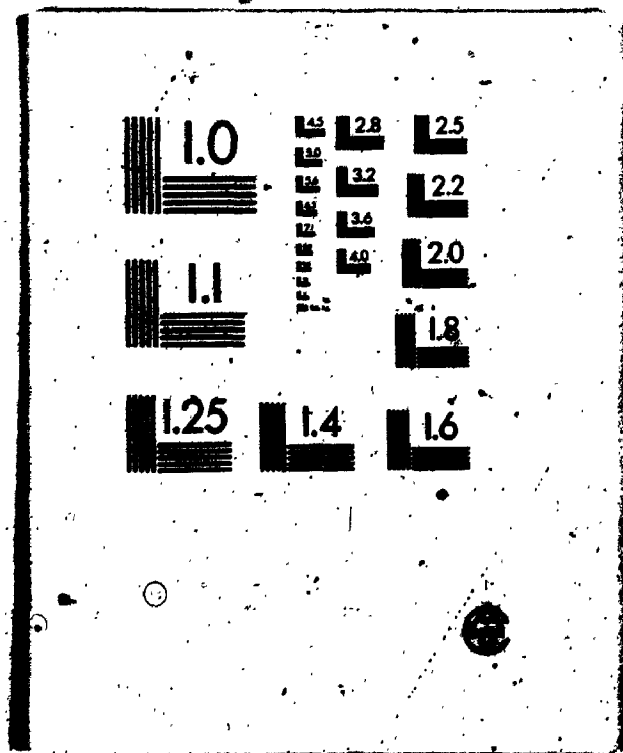
from critical evaluation." [EOK, p. 67]

I think Levi is right in this regard.

Both Kuhn and Levi incidentally have suggested a list of contextual factors pertinent to epistemic decisions. Kuhn regards epistemic values like simplicity, scope, fruitfulness and accuracy as important factors for scientific change. Levi's list presumably is much broader. In addition to Kuhn's list of epistemic values, Levi would add background information, the set of potential answers, the epistemic utilities of these answers, the question under investigation the degree of caution exercised by the investigator relative to certain cognitive demands and so on as relevant items affecting a cognitive decision. As we shall examine these items later on, we are not going to discuss them here. Suffice to note here is that the list proposed by Kuhn and Levi seems quite reasonable, it largely consists of factors which are commonly accepted in the literature.

Once again, though different investigators would attribute different weights to both the values and evidence, thus generating different preferences; such differences nevertheless should be open to critical examination. And to the extent that they are open to rational examination, they are not arbitrary. Admittedly, to have all members of a scientific community adhering to an unambiguous preference

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structure is a very difficult thing. Very often, I suppose, this is unnecessary. But the point here is that differences in preference do not necessitate arbitrariness.

Indeed, I suppose that the charge of arbitrariness is perhaps a result of a false dichotomy---either we have an unambiguous set of preferences applicable to whatever context, or the preference itself is arbitrary. I reject this dichotomy because it highly oversimplifies the situation. I suppose that between the two extremes depicted by the dichotomy, we still have a spectrum of choices. I do not think that we could have a single set of criteria applicable to different types of inquiry (in terms of goals). Different contexts, after all, require different criteria. Scientific inquiry is nonetheless a multi-objective activity. It is then legitimate to expect that it should consist of different goals and hence different epistemic criteria. Once again, this multiplicity of goals and criteria should not be seen as entailing subjectivism and arbitrariness.

KYBURG'S OBJECTIONS TO CONTEXTUALISM

In this section, we are going to examine some objections to local justification and contextualism. We shall try to show that these objections can be overcome.

The most serious challenger to the methodology of local induction by far is Henry Kyburg. Though Kyburg's original target was local induction, his argument could readily be used as an attack on local justification and contextualism as well. Kyburg's main criticism is nicely put together in the following passage:

"[the theories of local justification require] that opinions not differ too much (subjectivism), or that background knowledge not be at issue (objectivism). To the extent that these conditions are not satisfied---as they will not be in controversial instances of inductive argument--- the local philosophical theories must be capable of being broadened. A theory which was capable of being broadened in all respects and without limit would be a global theory. Thus a local theory will be either redundant and unnecessary (when everybody is agreed that the inductive conclusion follows from the evidence for it, 'conclusion,' 'follows,' and 'evidences' being construed broadly enough to include the case that the 'conclusion' is a certain degree of belief in the hypotheses, the 'evidence' is a set of degrees of belief in other statements, and the 'following' consists in adherence to coherence and a principle of belief change.) or it will be impotent to

resolve disagreement (when our prior degrees of belief differ too much, or when our background knowledge differs to the extent that the objectivistic test appropriate to your circumstances differs from the objectivistic test appropriate to mine.)" [Kyburg 1976, p. 214, emphasis added]

From the passage just quoted, Kyburg in effect puts forward two objections against local justification. The first one states that local justification assumes that there is no disagreement on the background knowledge of the investigators in the particular context of inquiry. That is, local methodologists suppose that in a local context, "the investigator's findings and beliefs that are relevant to the problem at hand are not likely to be questioned by any participant in the inquiry..." [GWT, p. 4] If this assumption failed, Kyburg argues, then local approach simply could not work. That is, if the participants disagreed on either the prior degrees of belief or background information, then the local approach simply could not help us to arrive at a common answer. In fact, Kyburg is referring to two distinct kinds of disagreement. The first kind of disagreement occurs, for example, when the background knowledge differs to the extent that the objectivistic test appropriate to one's circumstance differs from the test appropriate to the other. [Ibid. p. 214]

The second kind of disagreement, on the other hand, concerns

"the bearing of a specific body of evidence on a specific set of hypotheses. [Ibid. p. 193] In either cases, Kyburg contends, local justification is impotent to resolve disagreements.

THE IMPOTENCE OBJECTION

Let us refer to Kyburg's first objection as the impotence objection. With regard to the two kinds of disagreement alluded to by Kyburg, he seems to suggest that the local approach is required to resolve conflicts of this sort. But we might ask whether this requirement is itself justified. Surely, to ask this question is to raise a very fundamental question about the proper role of local approach. Nevertheless, it seems pertinent here to refer back to Levi to see what he has to say on this issue. Recall that local justification is the justification of conclusions relative to a given body of evidences and a set of presumptive answers to the problem raised. The intended function of the methodology of local justification, according to Levi, is "the establishment of criteria for determining which of the relevant answers to a given question, on the evidence available, is the best." [GWT, p. 5] Surely, there is no guarantee that within a local context there is consensus on matters relating to background information, potential answers and so on. But in

normal cases, especially in the case of Normal Science (in Kuhn's sense), disagreements on these matters are not serious even if they do exist. Hence, they do not normally constitute any real difficulty to the local methodology. Indeed, as pointed out by Kuhn [1970], there is amazingly enough consensus on those matters mentioned by Kyburg. Suppose, for the sake of argument, that disagreements of the sort referred to by Kyburg do occur, does it follow that local methodology is shown to be incapable of handling them? The answer to this question seems to be No. To say this we are not saying that local methodology is capable of handling this problem. Nor are we saying that local methodology could be shown to be competent. Our point here is simply that local methodology is not designed to do such a job. If a method were not designed to do a certain job, we certainly would not call it impotent if it "fails" to do the job. The same holds for the local methodology. If there were dissensus about evidence and potential answers and background information, some methods should be called for. These methods, among other things, might as well include local methodology, perhaps in an oblique way. So far it is not yet proven that local methodology is in principle incapable of resolving conflicts of this kind. After all, to attribute the "failure" in resolving dissensus solely to the responsibility of local methodology is surely a misplacement of responsibility. Local methodology, however, is objectionable if it precludes any possible method to

reconcile disagreements. Nevertheless, there is no reason for us to suppose that methods of resolution of conflicts are inherently incompatible with local methodology.

Referring back to the two kinds of disagreement---background information disagreement and evidential status of answers disagreement---alleged by Kyburg. The result of having substantive disagreement on background information, according to Kyburg, is that objectivistic test appropriate to one's circumstances differs from the objectivistic test appropriate to the other. Presumably, Kyburg wants to draw from this that the difference in tests in such situations would yield different answers. Otherwise, the differences in testing could not constitute any forceful objection to local methodology. The problem however is: does the difference in background information necessarily result in the difference in tests, and hence the difference in answers? Kyburg seems to have no clear answer to this. Nevertheless, until this question is satisfactorily answered, this objection to local methodology lacks force. With regard to the second case of disagreement; admittedly, the disagreement in question is a fundamental one. It concerns the evidential support of a given answer. However, when disagreements of this sort occur, there simply is no easy answer. Problems of this sort have been examined by philosophers for decades and there is yet no satisfactory answer. The failure to offer

any satisfactory solution to this problem certainly cannot be seen as the fault of local methodology. Clearly, if localist failed on this count, globalist would not seem to fair any better. Therefore, localist could not be seen as the only culprit in this regard. In light of the above discussion, it seems that the disagreements alleged by Kyburg is not as damaging as he would want us to believe.

THE REDUNDANCY OBJECTION

Let us call Kyburg's second objection the redundancy objection. Kyburg maintains that when conflict occurred, the local methodologist would broaden the context as he saw fit. He accuses the localist of giving no criterion for doing so. If context could be arbitrarily broadened as one wished, Kyburg argues, then there would be no difference between a localist and globalist. In this sense, local methodology offers no real alternative to the traditional justification of beliefs. Kyburg says:

"[in case of conflict], we should be able to achieve some sort of agreement about what the evidence warrants, even if we don't start with the same body of knowledge or the same opinions. We can only apply the techniques of local induction to an ever wider set of circumstances; if, given my background knowledge, you agree that the

evidence support H, but you lack a certain item in my background knowledge, I should be able, using the same techniques of local induction, to convince you that the evidence warrants the inclusion of that item in your background knowledge, too. What we require, in short, is that the context of local induction be arbitrarily expandable. But if a theory of local induction has this property, it is already almost a global theory." [Kyburg Ibid. pp. 208-209]

To respond to Kyburg's criticism, there is no reason to suppose that the expandability of the context necessarily implies that the expansion itself is arbitrary. As noted by Kyburg, it is within a specific context that expansion of context is called for. Indeed, even within that particular context, one presumably has to offer reasons for expanding one's context. In a communal setting of scientific inquiry, this is particularly so. Different investigators, after all, would demand justification from each other if such situations really occurred. In fact, Kyburg himself has not given any argument to the effect that the expansion of context necessarily entails arbitrariness. On the other hand, to be able to expand one's context does not automatically make one a globalist. In any specific context of inquiry, inasmuch as the context invoked was something short of the totality of our beliefs, localist would still

remain a localist. Any such expansion nonetheless would be perfectly compatible with his own position. In view of this consideration, we do not think that the local methodology is not an alternative to the global approach. Therefore, local methodology is not as redundant as Kyburg wants us to believe.

COMMENTS

I suspect that the controversy here may be the result of the ambiguous uses of the concept of context. Insofar as his arguments go, Kyburg seems to use the notion of context to refer to background knowledge or system of beliefs an investigator has at a given time. Levi, on the other hand, uses "context" to refer not only to background knowledge, but to questions and answers as well. Indeed, it is the inclusion of questions and answers, among other things, that clearly distinguishes local methodologists from global ones.

However, this is not to say that the distinction between local and global induction lies on the scope and content of the context--- globalist context refers to total background knowledge or system of beliefs and localist context refers to a more restricted body of background knowledge plus questions and answers. Insofar as background knowledge is regarded as an essential part of epistemic

context, there is no denial that both globalist and localist would agree that scientific acceptance is in a sense essentially contextual acceptance. That is to say, acceptance has to be relativized to background knowledge, among other things. But the real difference between localist and globalist apparently lies not so much on the contexts each invoked as on their respective procedures of justification. In global induction, justification of acceptance is done without any reference to any question (or questions), but only relative to the total system of beliefs. Local induction, in contrast, is the inductive acceptance relative to specific questions and answers.

Kyburg, in his argument against local induction, seems to have paid little attention to this difference but only concentrated on the difference in context. His understanding of local induction thus seems to be different from Levi's version. Therefore, even if his argument is sound, it will not affect Levi's position.

In the rest of this chapter, we shall examine other epistemological concepts relating to contextualism.

CORRIGIBILISM AND OTHER CONCEPTS

With regard to the justification of knowledge, Levi distinguishes between two versions of the justificationist

approach to knowledge. According to him, one version tries to justify knowledge in terms of its biological, psychological and social origins. People subscribing to this view may be called externalist. Others try to justify knowledge in terms of some indubitable, epistemically certain first principles. Those subscribed to this view may be called foundationalists. Levi says:

"[The first version of] justification is alleged to required tracing of the biological, psychological, or social causes of belief to legitimating sources. Another view denies that causal antecedents are crucial. Beliefs become knowledge only if they can be derived from impeccable first premises according to equally noble first principles." (EOK, p. 1)

Epistemologically speaking, Levi regards both view as untenable. He argues that more attention should be given to the improvement of knowledge rather than its justification. He says:

"Epistemologists ought to care for the improvement of knowledge rather than its pedigree. They ought to ask what X (who may be a person or a group) should do, given his knowledge at a time t, to render that knowledge more efficient in performing

its functions." [Ibid.]

Obviously, speaking of improvement instead of justification by no means makes the epistemologist exempt from the question of justification. The question of justification, as Levi himself admits, still remains. That is, one still has to justify why knowledge needs improvement and how it is improved. For example, we need to provide answers to questions like: (1) why should one modify one's knowledge in the first place? (Usually one needs not modify one's corpus unless one has good reasons for doing so.) (2) Why is it that hypothesis h is added to the corpus? (3) Why is it that hypothesis h is removed from the corpus? etc. The point here, after all, is not that Levi objects to justification per se, but that he opts for a shift in the locus of justification, i.e., the justification of corpus revision.

A knowledge corpus needs revision because what has been or will be accepted into the corpus might later be found false. This is presumably the fundamental reason that corpus needs revision from time to time. Levi's idea of corpus revision presupposes a version of corrigibilism. According to the doctrine of corrigibilism, knowledge is fundamentally corrigible---no element within the corpus of knowledge is immune to removal. In contrast with corrigibilism, incorrigibilism asserts that in order to have

knowledge at all, the basis of knowledge should be incorrigible. While corrigibilism usually associated with a non-foundationalist epistemology, incorrigibilism is normally connected with foundationalism. The latter claims that either some a priori first principles or some neutral observation basis should be taken as the solid foundation upon which knowledge is built. Those using first principles as the basis (e.g. Descartes) are called "top-down" foundationalist. Those using neutral observation basis (e.g. Logical Positivists) are referred to as "bottom-up" foundationalist. They both hold that the basis of knowledge is immune to revision.

Levi, like any other corrigibilists, flatly denies that there is any such incorrigible basis. With Perice, he contends that knowledge itself is self-correcting and requires no incorrigible basis. Despite being a corrigibilist, however, Levi also holds some form of incorrigibilism. It is interesting then to see how the two can be combined together without incompatibility. First, we have to understand what form of incorrigibilism Levi is holding.

Levi divides the knowledge corpus into two parts---the corpus K and the urcorpus UK. Roughly speaking, K consists of empirical statements of various sorts. UK, on the other hand, contains only statements of set theory, mathematics and logic. They are, so to speak, formal statements.

Levi's incorrigibilism states that every items of UK, unlike those in K, is incorrigible, i.e. they are 'immune' to revision. Since items of UK are formal truths, it is tempting to identify them with conceptual, a priori, or analytic truths. Levi, however, cautions us not to do so. Conceptual and analytic truths, though regarded by Levi as constitutive of a conceptual framework, are still revisable. [EOK, p.7] Since the items constitutive of conceptual frameworks are corrigible, they therefore can not be identical with items within UK. But this seems to be a very strange position indeed. If logic, mathematics and set theory, whose statements are supposed to make up UK, are not regarded as constitutive of conceptual frameworks; what else can? Nevertheless, Levi's view here is quite ambivalent. On some occasions, he differentiates UK from conceptual framework, elsewhere he identifies the two. [EOK, p.8]

Moreover, the way that Levi regards UK as incorrigible is also open to objection. Levi never gives any argument for taking the items of UK as incorrigible, he just assumes it. To my knowledge, he never gives any reason for making such an assumption except by saying that he does not want to discuss changes in UK (which he calls conceptual change). He says:

"I am concerned with changes in knowledge which are changes in [the corpus K] and I do not wish to

countenance changes which allow the falsity of truths of first order logic, set theory, or mathematics to be serious possibilities." [EOK, p.

7]

But clearly nobody would think this is a good argument. As a matter of fact, I don't see why Levi needs incorrigibilism given his commitment to corrigibilism. No harm is created by regarding all items within both K and UK as corrigible while allowing degrees of corrigibility among these different items within the corpus. For example, one would allow items in UK to have a low degree of corrigibility whereas items in K have a relatively high degree. Such a move, it seems, would have been more coherent to the spirit of corrigibilism without creating unduly artificial distinctions.

EPISTEMOLOGICAL INFALLIBILISM AND KNOWLEDGE AS STANDARD FOR SERIOUS POSSIBILITY

In addition to his corrigibilism, Levi also holds a version of infallibilism. Once again, it is interesting to examine how this combination, like the previous one, is at all consistent. To begin with, the version of infallibilism Levi holds is called "epistemological infallibilism". [EOK, p. 13] (EI for short). Levi maintains that EI is only a

natural consequence of the view of taking knowledge as a standard for serious possibility. He says:

"An immediate consequence of the thesis that X's corpus at t serves as his standard for serious possibility at t is that, according to X at t , no item in his corpus at t is possibly false in the sense of serious possibility. If h [is in] $K_{x,t}$, then h is infallibly true according to X at t in a straightforward and important sense." [EOK, p. 13]

That is, EI asserts that no item in X corpus at t can be regarded as false in the sense of serious possibility. Alternatively, every item in K is infallibly true for X at t . A clear understanding of the claim of EI surely depends on the understanding of the notions of serious possibility and knowledge as standard for serious possibility.

For Levi, X's knowledge at time t should be viewed as a resource in his subsequent inquiries and deliberations. What is meant, one would ask, by saying that knowledge is a resource for inquiry? Briefly, to the extent that it can provide evidence sentences as a basis upon which other sentences are judged, (in terms of their acceptability into the corpus), the corpus K is a resource for inquiries. Levi has coined a nicer phrase for this, he calls it "knowledge

as a standard for serious possibility," and elaborates it as follows:

"X's knowledge at t serves as a standard for distinguishing truth-value-bearing hypotheses whose truth is a serious possibility according to X at t from those whose truth is not a serious possibility." [EOK, p. 3]

In flipping a coin, for example, one would entertain as possible that either it will land heads up, it will land tails up, or, it will land on its edge. However, one would not entertain as possible that the hypothesis that the coin will suddenly "evaporate" into the air or that it will keep staying in the air and will never land on the ground. To be sure, these two sets of possible hypotheses are logically possible hypotheses. In real life, nevertheless, we would never take the second set as serious and may have good reasons for doing so. For one thing, the second set of hypotheses is not seriously considered because it patently violates some fundamental physical laws. It seems that physical laws here do serve as a standard for distinguishing hypotheses for serious consideration from those that are not. The question is: do they also serve as a standard for distinguishing what are seriously possible from those that are not? The answer seems to be that they, as items of K, can only be regarded as necessary but not sufficient

conditions for determining serious possibility. The other factor, among other things, is the agent's credal or subjective probability. Levi says:

"...judgments of subjective or credal probability are intimately related to evaluations of hypotheses with respect to serious possibility. If hypothesis h bears positive credal probability according to X at t, X should evaluate the truth of h as a serious possibility." [Ibid.]

This statement, however, should not be taken as true without qualification. For in some cases, Levi points out, a hypothesis having a zero credal probability may also be a serious possibility. For example, a certain magnitude for X may be representable by a real number in some interval, but X may not know what the number is. Hence, the related hypotheses may be taken as serious possibility while being assigned 0 credal probability. [EOK, p.4]

One distinction which merits attention here is the distinction between serious possibility and relevant possibility. According to Levi, serious possibility is primarily determined by virtue of X's corpus at t. He says that "once X's corpus at t is fixed, the distinction between what is and what is not a serious possibility...should be fixed." [n. p.4] Relevant possibility, on the other hand,

is not fixed relative to X's corpus, but rather to X's problems, goals and values. He says:

"...even if the corpus remains fixed, a change in X's problems, goals, and values could alter his assessment of which serious possibilities are relevant possibilities..." [Ibid.]

For example, if X's problem is to decide whether in a toss of coin it will land heads up or land tails up. Hypotheses: "the coin will land heads up and there will be a snow storm tomorrow" and "the coin will land heads up and Joe Clark will be defeated in the next election", though serious possibilities, should not be counted as relevant possibilities with respect to X's problem. After all, Levi himself admits that the above still falls short of being an adequate explication of the notion of serious possibility. He offers a rather vague characterization of the notion as follows:

"h is a serious possibility according to X at t if and only if h is consistent with his corpus of knowledge at t." [EOR, p. 5]

Obviously, this definition is simply too broad to be useful. Using consistency as the relationship between the corpus and serious possibility does not work because it fails to

preclude logical possibilities. It seems that a more restrictive definition is required. For our purposes, however, the discussion in this paragraph seems enough to give us an intuitive understanding of the concept of serious possibility.

Despite Levi's endorsement of infallibilism, he maintains that it can coexist with fallibilism. He says:

"Of course, X may consistently acknowledge that items he accepted in his corpus at previous times or will assume in the future are possibly false. He is not committed to the view that whatever he has endorsed in the past or will accept as evidence in the future is infallibly true. But at t, X is committed to the view that whatever he assumes as part of his corpus at t is infallibly true." [EOK, p. 13]

The first two statements in the quoted passage clearly express a version of fallibilism that epistemologists usually hold. The point Levi wants to make here is that his epistemological infallibilism not only is entailed by his view that knowledge be treated as standard for serious possibility and as a resource for inquiries and deliberations, but that it is also not incompatible with fallibilism. It seems that the version of infallibilism to

which Levi committed himself is a kind of myopic infallibilism. That is, h is regarded as infallibly true only relative to a specific context of inquiry at a specific time frame. At a later time, h may be found to be false given new evidence is indeed a perfectly natural thing. In this sense, Levi maintains that this version of infallibilism is basically consistent with fallibilism. Once again, let us see how Levi explains his own position:

"...my position remains fallibilist insofar as fallibilism is equivalent to corrigibilism---the thesis that X's standard for serious possibility...is sometimes legitimately subject to revision." [Ibid. p. 18]

"In advocating ...epistemological infallibilism, I also do not intend to claim infallibility for any source of information... even X should acknowledge that when h is not possibly false in the serious sense directly pertinent to the conduct of deliberation and inquiry, it remains, nonetheless, logically possible that it is false (provided that h is not a logical truth). [EOK, pp.18-19]

These passages suffice to show that Levi's infallibilism is quite innocuous.

One final remark before we finish our discussion of this section. For Levi, the concepts of certainty and infallibility are not the same as the concept of incorrigibility. Though incorrigibility implies infallibility, the converse does not hold. The concept of incorrigibility of an item is defined in terms of its removability from the corpus, it is not defined merely in terms of truth. Hence, it is a far stronger notion than infallibility. Levi explains:

"From X's point of view...all assumptions in his corpus are equally certain and infallible. Yet some are more vulnerable to removal than others. Some assumptions are maximally certain and infallible, and, yet, are highly corrigible. Others may be equally as certain and yet eminently incorrigible (e.g. items in the urcorpus)" [EOK, p.61]

"Certainty and infallibility are one thing, corrigibility is another." [EOK, p. 27]

This concludes our discussion of Levi's epistemological position.

FOOTNOTES

1. Admittedly, this is a rather trite fact concerning scientific practice. It is also true that acceptance acts in general are highly restricted in scope as well, regardless of whether the context is local or not. In fact, even traditional induction, which is invariably global in scope, is also context dependent in another sense, viz. the role of background knowledge. Therefore, I suspect that the distinction between global induction and local induction has to be drawn not in terms of context but in terms of the procedures of acceptance. We shall come to that when we examine Kyburg's arguments.

CHAPTER VI

LEVI'S EPISTEMIC DECISION MODEL

In this chapter, we are going to look more closely into the structures of the two models of epistemic decision making which Levi has developed in the last decade or so. The first model, originally proposed in his GWT, was designed for a number of relatively limited cognitive purposes. In his EOK, however, a more comprehensive model has been constructed for a wider scope of application. Nevertheless, as we shall show later on, the second model is not an entirely new system but only an extension of the first one. Before the emergence of the second model, various modifications have been done on the first model, and the subsequent changes were incorporated in the later model. In what follows, we shall try to present Levi's system of epistemic decisions systematically without detailing the historical genesis of it. We shall first examine Levi's system as presented in GWT, then considerable attention will be given to his theory of epistemic utility, especially his pragmatic treatment of the utility of information. Hempel's theory of epistemic utility is introduced here as a contrast to Levi's theory. Indeed, it is through such comparison that an assumption of Levi's preference structure is unraveled. Meantime, Levi's various modifications of his original proposals are reviewed and the

reasons for such modification are presented. After this, we shall examine his second model.

The objectives of model I, according to Levi, are estimation, prediction and generalization. The decision problem, as conceived by Levi, is the acceptance of statements as best answers to specific questions. Model I, as it were, is the epistemic decision model within which the best answer to a question can be determined. To begin with, model I presupposes a language L, which is the linguistic framework whereby epistemic beliefs are expressible as sentences or statements. Since Levi's model deals particularly with the acceptance of beliefs as sentences, a language L certainly is required. The structure of language L, except for a few minimal requirements, is largely left unspecified. Indeed, Levi only requires L to have an underlying logic similar to that of the lower predicate calculus with identity. Meanwhile, language L is allowed to include as many extralogical terms and axioms (which include those of set theory and mathematics) as needed. The rationale for such minimal requirement seems clear. The logic presupposed is indeed a well-worked out system. It has a clearly defined structure and relationships like consistency, deducibility and so on which could serve a lot of methodological purposes.

THE MEANING OF ACCEPTANCE CLARIFIED

We have been using the term "acceptance" as if its meaning is well understood. In fact, the meaning of acceptance of a sentence is by no means unequivocal nor uncontroversial here. It is then proper for us to give a brief clarification of the concept intended in this context. Within Levi's model, acceptance is understood in a relative sense---a sentence is accepted on basis of some other sentences, among other things. The set of sentences which serves as a basis upon which other sentences are accepted is referred to by Levi as evidence sentences, or simply, evidence. For Levi, this set of sentences must not only be accepted as true, but be accepted as evidence. But what is the difference between the two ? According to Levi, to accept a sentence h as true means to take h as true in the Tarskian sense.¹ For Tarski, a sentence h is true if and only if the states of affairs which h represent obtains--- "snow is white" if and only if snow is white. Having a sentence accepted as true, however, does not imply that it is also accepted as evidence. A true sentence is also accepted as evidence only if it is used as a basis upon which other sentences are judged as acceptable or not. If this is so, then clearly acceptance as evidence is a stronger notion than acceptance as true. Having a sentence accepted as evidence implies that it is accepted as true, but the converse is not true. To use Levi's example,

a medical researcher might think with good reason that a certain drug is safe from harmful effects, yet he may still have good reasons to continue to test its safety. Now he may think the drug safe, yet in the course of testing it, this certainly cannot be taken as evidence. (GWT, p. 28) Elsewhere, Levi identifies acceptance as true as mere acceptance.

Another sense of acceptance merits attention here is that we are not using acceptance in the psychological sense, but only in the rational sense. By this we mean that we are not concerned with what an agent actually accepts, rejects, or suspends judgements on in an epistemic situation. Rather, the problem of our concern is: given such and such conditions, what ought an agent accept in a given epistemic context. In other words, it is the normative aspect of cognitive acceptance which concerns us here. As we have mentioned before, we shall try to replace the talk of inference by the talk of decision, and hence acceptance; so what we are referring to as inductive acceptance may also mean inductive inference. Once again, the inductive acceptance of a sentence means the acceptance of a sentence which is not entailed by the evidence. In later sections, we shall try to show that acceptance is not only relativized to evidence but also to a host of other factors--- the knowledge corpus, ultimate partitions, cognitive desiderata, credal probability and the index of caution.

THE STRUCTURE OF EVIDENCE SENTENCES AND DEDUCTIVE COGENCY

If acceptance of a sentence as an answer to prediction, estimation and generalization has to be relativized to a set of evidence sentences, does it mean that we have already accepted the latter sentences? As mentioned before, evidence sentences are not only accepted as true, they are accepted as evidence. This is exactly why they are referred to as evidence sentences. What we have to know here is the structure of this set of sentences. Levi maintains that the set of evidence sentences should satisfy certain requirements. They are:

- (1) it is deductively closed---contains all deductive consequences in L of sets of sentences of the set;³
- (2) it contains a deductively closed set consisting of all logical, set-theoretical and mathematical truths expressible in L;
- (3) it is consistent. [GWT, p. 26]

Elsewhere, Levi refers to this set as a knowledge corpus K.

These conditions, incidentally, can be combined together to form a principle called "the principle of deductive cogency" [Ibid]. So, satisfying these conditions is tantamount to satisfying the deductive cogency principle. Meanwhile, the principle itself seems to be a rather stringent standard to be satisfied. Presumably, even an

ideal agent may not be able to live up to or capable of living up to its requirement. The reason is simple. In normal circumstances, an agent may not always be able to determine whether a given sentence is logically true or not, nor can he easily decide whether the sentences he has accepted or is going to accept are consistent with each other. Therefore, to require that the agent must satisfy deductive cogency seems to be unreasonable and unnecessary. To weaken this requirement, Levi maintains that instead of demanding the agent to fulfil the principle, we only demand him to be committed to such requirements. This means that he is required to be committed to accepting logical truths, the deductive consequences of sentences accepted and maintaining consistency etc. whenever he is capable of doing so. Presumably, interpreting the principle as the requirement of commitment has one apparent merit. That is, it allows the possibility that an agent, though sometimes failing to meet the requirement, remains rational. The failure to observe the deductive cogency requirement may, according to this construal, readily be attributable to factors like the lack of skill in logical computation or the lack of imagination on the part of the agent, but not to the abandonment of his commitment. On the other hand, if the agent understands the requirements well and has considerable skill in executing them, we can legitimately regard him as betraying his commitment if he refuses, for example, to accept conclusions deducible from his accepted sentences.

[AR, p. 22] Thus, the mere failure to identify logical, set-theoretic or mathematical truths, or the failure to detect inconsistency and so on does not by itself constitute any violation of the principle. Rather, it is the failure to uphold one's commitment that counts as a violation. In this sense, the principle of deductive cogency understood here is clearly a normative principle. Indeed, insofar as it provides rational constraints on what is to be accepted as evidence, it can also be viewed as a kind of rationality principle.

The satisfaction of deductive cogency also gives some interesting properties to the structure of the set K:

- (1) K is an infinite set---it contains an infinite number of sentences as its members;
- (2) If there is a strongest sentence in K, that sentence is the one which entails every element of K in L;
- (3) If K is axiomatizable with the help of a finite number of axioms, then there is at least one strongest sentence in K---i.e. the conjunction of these axioms.

Though K is axiomatizable, Levi does not require that K be axiomatizable. [GWT, p. 29]

ULTIMATE PARTITIONS AS ANSWERS TO QUESTIONS

Just as the acceptance of sentences has to be relativized to a body of evidence sentences, the acceptance of sentences has also to be relativized to a specific question the agent raised. With respect to that specific question, a set of sentences is delimited as potential answers to the question. Levi refers to this set as ultimate partitions. In a specific context of inquiry, it is not necessary to suppose that X should consider all logically possible answers to his question. What normally happens is that X considers only a relatively restricted set of answers---the relevant answers---to his question. The notion of ultimate partitions is nonetheless devised as a formal representation of this set of relevant answers to a question. Levi characterizes ultimate partitions as follows:

"Let U_e be a finite set of n sentences in L such that each element of U_e is consistent with the set of evidence sentences (sentences entailed by $b \& e$), and such that $b \& e$ entails at least and at most one element of U_e is true. (If total evidence is b , a set of the sort described will be called U .) The elements of U_e are to be arranged in some definite alphabetical order." [GWT, p. 34]

Here \underline{b} refers to background information, \underline{e} the newly available evidence. Relative to such ordering, on the other hand, we have a set M_e of sentences which contains the following sentences as subsets:

- (i) S_e ---the sentence formed by disjoining every element of U_e ,
- (ii) C_e ---the sentence formed by conjoining every element of U_e ,
- (iii) G_e ---the sentence formed by disjoining \underline{m} elements of U_e ($1 < m < n$) where each disjunct appear once and only once in alphabetical order. There are $2^n - 2$ such sentences.

Observe also that only 2^n sentences covered by (i)-(iii) are elements of the set M_e generated by the set U_e relative to the total evidence $\underline{b \& e}$.⁴

It is clear that U_e not only represents the set of relevant answers but also helps to delimit the relevant answers to those sentences that are logically equivalent to the sentences of the set M_e constructed out of U_e . Levi's precise definition of ultimate partitions is as follows:

"An ultimate partition U_e relative to the total evidence $\underline{b \& e}$ is a set of sentences in \underline{L} exclusive and exhaustive relative to $\underline{b \& e}$, such that no

element of U_e is entailed by b_e and such that each relevant answer considered by the investigator using U_e is logically equivalent to an element of the set M_e generated by U_e ." [GWT, p. 35]

Questions may be raised as to whether there is a general criterion for determining ultimate partitions. While acknowledging the difficulty of the question, Levi admits that he has no answer to it. In GWT, he just takes the determination of ultimate partitions as given.

To facilitate understanding, let us use an example to illustrate the idea of an ultimate partition. Suppose a person A wants to predict in a horse race which of the three horses X, Y, and Z will win. His total evidence is b_e . His ultimate partition U_e , accordingly, consists of the following three sentences: "X will win," "Y will win," and "Z will win." On the other hand, U_e also yields a set M_e comprising the following sentences:

- (i) X or Y or Z will win (S_e)
- (ii) X or Y will win.
- (iii) X or Z will win.
- (iv) Y or Z will win.
- (v) X will win.
- (vi) Y will win.

(vii) Z will win.

(viii) X and Y and Z will win. (C_e)

Accepting S_e as the answer given the evidence is the same as refusing to accept any sentence as true relative to the evidence other than the evidence. To do this is equivalent to suspending one's judgement. On the other hand, to accept C_e as the answer is to accept a contradiction; because C_e is inconsistent with b & e. As the remainder of M_e are G sentences, the acceptance of either one of them would provide different degrees of information to that already conveyed by the evidence.

When considered together with deductive cogency the introduction of ultimate partitions nonetheless has one implication that deserves notice. Recall that deductive cogency is restricted to sentences that are accepted relative to a body of evidence. The question here is whether deductive cogency should also be made relative to ultimate partitions. This question, I suppose, could best be answered by considering an example suggested by Levi. [GWT, p. 37-38] Once again, suppose in predicting the outcome of a horse race, person A takes his ultimate partition as consisting of the sentences "X or Y will win" and "Z will win." Person B, on the other hand, uses a partition that consists of "X will win" and "Y or Z will win." Both A and B think, given the available information,

that the chance of Y winning the race is high. Accordingly, A will be expected to predict that "X or Y will win" and B to predict that "Y or Z will win." The question here is whether they could be allowed to combine their conclusions to get the conclusion that Y will win? According to Levi, such a move is not permissible because the sentence "Y will win" is not logically equivalent to any element in the set M_e generated by A's ultimate partition or to any element in B's M_e . In other words, "Y will win" is not counted as a relevant answer by both A and B. In light of this consideration, deductive cogency is limited not only by the evidence, but also by the agent's commitment to his ultimate partition.

LEVI'S THEORY OF EPISTEMIC UTILITY

To be able to apply decision theory in cognitive situations, epistemic utilities are required. The epistemic utility, as it were, is supposed to represent epistemic merits or values of cognitive outcomes. Technically, such epistemic utility is supposed to be a real-valued function defined over a boolean algebra generated by the elements of $U (U_e)$, i.e. $M (M_e)$.

To my knowledge, Levi offers several formulations of his theory of epistemic utility. For the benefit of our exposition, a simpler formulation is preferred here. Levi

constructs his theory by first laying down some postulates and definitions, then theorems are derived. The postulates, according to Levi, are supposed to capture what he regards as reasonable intuitions concerning epistemic preferences. In GWT, Levi only invokes two postulates. Elsewhere,⁵ however, more postulates are introduced.

In this section, we shall discuss Levi's postulates and some related theorems. Nevertheless, no proofs of these theorems are provided since they are rather obvious. Finally, we shall introduce Levi's inductive acceptance rule.

The two postulates of Levi's epistemic utility theory are expressed as follows: [GWT, p.76]

- (1) Correct answers ought to be epistemically preferred to errors.
- (2) Correct answers (errors) that afford a high information content ought to be preferred to correct answers (errors) that afford a low information content.

It is clear that postulate (2) is composed of two subpostulates. For clarity of exposition, we unpack it as follows:

- (2.1) Correct answers that afford a high information content ought to be preferred to correct answers that

afford a low information content.

(2.2) Errors that afford a high information content ought to be preferred to errors that afford a low information content.

The intent of stipulating these postulates seems clear. By means of them, we obtain a preference ordering of the two epistemic attributes ---truth and information---of cognitive outcomes. Indeed, with respect to the goal of obtaining true and informative answers, these postulates seem to very well preserve our intuitions about epistemic preference. Postulate (2), however, seems to presuppose some kind of independence between the attribute of informativeness and truth. It assumes in effect that informative answers are to be preferred no matter what their truth values are. This assumption seems to hold in the choice of correct answers. However, if the choice is among errors, it is arguable whether this same assumption will still hold. We shall come to this question when we discuss Hempel's theory.

Levi proposes two functions to represent information content and truth of sentences respectively. Let function $T(h, t)$ be the utility of accepting h as the best answer to a question given evidence b (h as strongest sentence via induction from b) relative to U without error, $T(h, f)$ be the utility of accepting h as the best answer given b relative to U with error. We have: ⁶

$$T(h, t) = 1$$

$$T(h, f) = 0$$

The utility of information content is represented by $C(h, x)$, where x is a variable which takes true or false as its values. Levi wants the C-function to be the utility of the informational value of the elements of $U(U_e)$ independent of their truth values. The C-function is defined as follows:

$$\bar{C}(h) = 1 - m(h)$$

The m -function here is a normalized probability measure defined over the boolean combinations of elements of $U(U_e)$ and sentences equivalent given $b(b \& e)$ to such combinations. More explicitly, m is a function which assigns real numbers to elements of the set $M(M_e)$ generated by $U(U_e)$ which satisfies the following conditions:

(a) For every element a in U , $m(a) > 0$

(b) Let a_1, a_2, \dots, a_n be elements of U ,

$$\sum_{i=1}^n m(a_i) = 1$$

(c) $m(C) = 0$

(d) If G is in M and a_1, a_2, \dots, a_k are elements of U which are disjuncts in G ,

$$m(G) = \sum_{i=1}^k m(a_i) \quad [\text{GWT, p. 69}]$$

Using these two functions---T-function and C-function---Levi tries to construct another utility function $V(h,x)$ which represents the combined attribute of both truth and informativeness. As expected, $V(h,x)$ should be some function of both $T(h,x)$ and $C(h,x)$. Besides, Levi also wants these three functions to be definable only up to a positive linear transformation. What we are going to do now is to examine the conditions proposed by Levi as constraints on the T-functions and C-functions as well as the way in which they are combined together to form the V-function. Once again, these conditions are supposed to capture what Levi regarded as important intuitions concerning epistemic preferences.

The first intuition is something like this. If G is equivalent given $b \& e$ to F , accepting G as strongest answer via induction from $b \& e$ yields the same answer as accepting F as strongest answer from $b \& e$. Hence the epistemic utility of accepting G as best answer when it is true (false) should be the same as that of accepting F as best answer regardless of which epistemic utility function is used. [II, p. 374] Condition (A) is supposed to represent this idea:

(A) G is equivalent given $b \& e$ to F which is a member of M_e ,

$$(1) T(G,x) = T(F,x)$$

$$(2) C(G,x) = C(F,x)$$

(3) $V(G, x) = V(F, x)$, where x is a variable whose values can be true or false.

According to Levi, (A) also has the effect of excluding any measure of information which is based on prior probability. To see this, let us have $\text{cont}(H) = p(-H)$. If $\text{cont}(H)$ is a proper measure of information defined over the elements of U , any linear transformation of it should be substitutable for the C-function. Now, though a logical truth $H \vee -H$ is logically equivalent to S_e given $b \& e$, it is nonetheless not logically equivalent, in general, to S_e . Thus, $\text{cont}(H \vee -H)$ will be 0 while $\text{cont}(S_e)$ in general will be positive: Therefore, $\text{cont}(H)$ cannot be substituted for $C(H, x)$ without violating condition (A). By parity of reasoning, other measures of information based on prior probability are not substitutable as well.

The next intuition to be considered is that when truth is the only desideratum, all true answers are to be treated as the same. This applies to all false answers as well. However, when true answers and false answers are considered together, true answers are epistemically preferable to false ones. These ideas are formulated in the following condition:

(B) For every H and G in M_e ,

(1) $T(H, t) > T(H, f)$

$$(2) T(H,t) = T(G,t)$$

$$(3) T(H,f) = T(G,f)$$

Notice that (B.1) incorporates the idea expressed in postulate (1).

In scientific inquiry, truth nonetheless cannot be considered as the only desideratum. In addition to truth, we also want the answers to be informative, among other things. Indeed, the search for information is sometimes so compelling that we are even willing to run the risk of making mistakes in order to obtain more information. For Levi, the search for informative answers has to be regarded as another legitimate goal of scientific inquiry as well. However, he maintains that the information obtained by accepting an answer to a question is independent of the truth value of the answer. That is, regardless of the truth value of a given answer, the information conveyed by it, if any, is the same. This idea is condensed in condition (C):

(C) For every H in M_e ,

$$C(H,t) = C(H,f)$$

The implication of (C) is clear---an agent who seeks solely for information faces no risk of error. His only objective is simply to obtain maximum information. Another agent who concerns only truth, in contrast, would face risks of

making errors. However, he can also abolish risks completely simply by suspending judgement. In the real world, nevertheless, these three single-objective agents seldom exist, if at all. In actual scientific inquiry, in particular, what we normally find are agents who are interested both in truth and information, among other things. Surely, such agents not only often face risks but also find them worth taking relative to the goal of obtaining both truth and information.

Let us go to the third condition. For a U_e with n elements, a M_e with 2^n elements can be generated. Since each element can be accepted correctly or erroneously (with the exception of S_e and C_e), there are 2^{n+1} elements to be assigned utility values. Suppose we partition the set of potential answers into two subsets---correct answers and errors. When the truth values are kept constant, changes in epistemic utilities are clearly changes in information only, i.e.

$$V(H,t) - V(G,t) = C(H,x) - C(G,x)$$

$$V(H,f) - V(G,f) = C(H,x) - C(G,x)$$

Since the unit for measuring the utility of information is arbitrary (i.e., the numerical values for the V-function can remain constant for arbitrary changes in the unit for measuring the utility of information), $V(H,t) - V(G,t)$

should be equal to $b_t (C(H,x) - C(G,x))$, where b_t is a positive constant. Likewise, $V(H,f) - V(G,f)$ should be equal to $b_f (C(H,x) - C(G,x))$ for positive b_f . Moreover, since we assume that the increments in the epistemic utility represented by V-function are increments in the utility of information when the elements compared have the same value, b_t should be equal to b_f . This is because though the C-function has an arbitrary scale, the choice of a scale should remain the same for all arguments of that function. [Ibid. p. 376] The above considerations are expressed in the following condition:

(D1) If H and G are members of M_e ,

$$(a) V(H,t) - V(G,t) = b_t (C(H,x) - C(G,x)), \quad b_t > 0$$

$$(b) V(H,f) - V(G,f) = b_f (C(H,x) - C(G,x)), \quad b_f > 0$$

$$(c) b_t = b_f = b$$

The set of potential answers can also be divided into subsets of elements K_1, K_2, \dots, K_m such that every element of the same K_i has equal information, but members of different K's have different information. If the elements of only one K_i are compared, increments in epistemic utility would be increments in the utility of truth value. Hence for any H and G in K_i ,

$$V(H,t) - V(G,f) = a_i (T(H,t) - T(G,f))$$

$= a_i (T(H,t) - T(H,f))$, where a_i is a positive constant of scale.

As expected, the constant of scale should be the same for all subsets K_i 's. To summarize the discussion in the above paragraph, we have the following condition:

$$(D2) \quad V(H,t) - V(H,f) > 0$$

(D2) in effect states that accepting H correctly is epistemically preferable to accepting it erroneously.

Though (D2), as it were, requires one to accept true answers rather than false ones; it does not exclude false answers from being preferable to some correct ones. To be able to do so requires another condition:

$$(E) \quad V(H,f) < V(G,t) \text{ , for every } H \text{ and } G \text{ in } M_e.$$

Condition (E), according to Levi, not only prescribes that errors are not to be preferable to correct answers, but also helps to impose certain limit on the relative importance of information as compared to that of truth. (E) in effect states that false answers, no matter how informative they are, should be less preferable than less informative true answers. Let me explain.

With regard to the unit of measurement of the two functions, Levi chooses the convention to assign the T-function (of true answers and errors) the values of 1 and 0 respectively. He also restricts the C-values in such a way that the maximum and minimum values possible would be the same as for the T-function. That is, he selects a unit of measuring C-values and T-values so that

$$C(W,x) - C(X,x) = T(X,t) - T(W,f)$$

where W is a member of M_e such that $C(W,x)$ is the maximum and X is a member of M_e such that $C(X,x)$ is the minimum. Relative to such a system of units, the ratio $(1-\alpha)/\alpha$ must be less than unity in order to satisfy condition (E).⁷ That is, the values of α must be restricted to the interval from .5 to 1 inclusive. In this way, (E) is able to impose a definite restriction on the relative importance which can be attributed to information and truth respectively.

In addition to the above conditions, i.e. (A) to (E), Levi suggests two more conditions for governing the utility of information. Condition (F) states that the information value of an element is the increasing function of its deductive power given the total evidence p&e:

(F) If G is an element of M_e and H_i is an element of U_e which is a disjunct in G ,

$$C(G \& \neg H_i, x) - C(G, x) > 0 \text{ [Ibid. p. 381]}$$

A theorem is readily derivable:

(T.9) For every distinct G and F in M_e , if G is deducible from $F \& b \& e$,

$$C(G, x) < C(F, x)$$

The last condition purports to take care of a case of the following sort. Suppose we have two elements G and G' of M_e which are compatible given $b \& e$ with an element H_i of U_e (i.e., H_i is a disjunct of both G and G'). The question here is whether the increment in the utility of information obtained by rejecting H_i in addition to all those elements of U_e incompatible with G given $b \& e$ differs from the increment obtained in the case of other element G' ? Condition (G) asserts that the rejection of an element of U_e provides the same increment of informational utility regardless of how many or which other elements of U_e are also rejected. This condition is as follows:

(G) Let G and F be elements of M_e and H_i be an element of U_e which is a disjunct in both G and F ,

$$C(G \& \neg H_i, x) - C(G, x) = C(F \& \neg H_i, x) - C(F, x)$$

From these conditions, some theorems are derived. For our purposes, only a few interesting and relevant ones are introduced. They are as follows: [GWT, p.377].

(T.3)

$$V(H,x) - V(G,x) = a(T(H,x) - T(G,x)) + b(C(H,x) - C(G,x))$$

(T.4a)

Let c be an arbitrary constant which may be set to 0,

$$V(H,x) = aT(H,x) + bC(H,x) + c$$

(T.4b)

Let $\alpha = a/(a+b)$, $V(H,x)$ is a linear transformation of $\alpha T(H,x) + (1-\alpha)C(H,x)$

It is obvious that the last theorem is just the weighted average of the utility of truth and the utility of information. It is therefore the utility of obtaining true and informative answers. The theorem can be broken up into two parts:

$$(a) \quad V(H,t) = \alpha T(H,t) + (1-\alpha)C(H,x)$$

$$(b) \quad V(H,f) = \alpha T(H,f) + (1-\alpha)C(H,x)$$

By substituting $C(H,x) = C(H) = 1-m(H)$ into both (a) and (b), we get

$$(c) \quad V(H,t) = \alpha T(H,t) + (1-\alpha)(1-m(H))$$

$$(d) V(H,f) = \alpha T(H,f) + (1-\alpha)(1-m(H))$$

Since $T(H,t) = 1$, (c) becomes

$$(e) V(H,t) = \alpha + (1-\alpha)(1-m(H))$$

Because $T(H,f) = 0$, (d) becomes

$$(f) V(H,f) = (1-\alpha)(1-m(H)) \text{ , where } 1/2 < \alpha \leq 1.$$

The restriction of α to the interval between $1/2$ and 1 is, again, to ensure that no error is preferable to a correct answer. The epistemic utility function obtained here, after all, is the weighted average of the C-function and the T-function. Notice that a and b , apart from being constants of scale (e.g. T.3), serve also as weights in the weighted sum in (T.4a) and (T.4b), provided that the units for measuring T-values and C-values are fixed. [*Ibid.* p. 378] Parameter α , as mentioned before, is supposed to reflect the relative importance attached to the demand for information and the demand for truth. As a and α go to 0 , the V-function approximates the C-function. Meantime, as b goes to 0 or α goes to 1 , the V-function changes to T-function.

The expected epistemic utility of accepting H as strongest via induction relative to $U(U_e)$ is calculated in the following way. The agent is supposed to have a credal

probability defined over the sentences of L and other boolean combinations of these sentences H_1, H_2, \dots, H_n be the set of sentences which is compatible with $\underline{b \& e}$ such that $\underline{b \& e}$ entails the truth of exactly one of these sentences. A credal probability is a real-valued function defined over the boolean combinations of these sentences which satisfies the following conditions:

- (1) $p(H, e) > 0$ for each of H_i 's.
- (2) $\sum_{i=1}^n p(H_i, e) = 1$
- (3) If G is incompatible with $\underline{b \& e}$, $p(G, e) = 0$
- (4) Let F be equivalent to a disjunction of m distinct H_i 's or to the conjunction of such a disjunction with $\underline{b \& e}$,
 $p(F, e) =$ the sum of the probability assigned to m H_i 's.
- (5) $p(H \& e, b) = p(H, b \& e) p(e, b)$ ⁸

Given the credal probability, the expected epistemic utility of H given \underline{b} , $EV(H)$ ($EV(H, e)$, H given $\underline{b \& e}$), is calculated by means of the following formulae:

$$(g) \quad EV(H) = \alpha p(H) + (1-\alpha)(1-m(H))$$

$$(g') \quad EV(H, e) = \alpha p(H, e) + (1-\alpha)(1-m(H, e))$$

Dividing (g) by α and then subtracting by $q = (1-\alpha)/\alpha$, we get

$$(h) \quad EV'(H) = p(H) - qm(H)$$

$$(h') \quad EV'(H,e) = p(H,e) - qm(H,e)$$

where $EV'(H)$ ($EV'(H,e)$) is a positive linear transformation of $EV(H)$ ($EV(H,e)$).

From (g) and (h), together with the Bayes' rule of maximizing expected utility and the rule for ties,⁹ an inductive acceptance rule is derived:

Rule (A) : Reject every element of U such that
 $p(H,e) < qm(H,e)$ ¹⁰

The parameter q is supposed to represent the index of caution exercised by the agent. When $q=0$, the demand for truth is maximum. If $q=0$, the agent will only accept what is entailed by the evidence. His only concern then is truth. Besides, he is also a skeptic. In this sense, the choice of the value of q helps to indicate the relative importance attached to the two desiderata of inquiry.

Let us recapitulate what we have done so far. We have tried to present Levi's epistemic decision model as conceived by him in his GWT and II. The objectives of model I are rather modest---estimation, prediction and generalization. As Levi only illustrates how his model works on very simple examples, whether it can work effectively on more complicated scientific cases is an open

question. Nevertheless, model I does succeed in displaying certain ingenuity and conceptual niceties which other kindred models lack. Also, we hope that the discussions of these sections could provide a solid basis for the subsequent inquiry into Levi's model II. However, before we come to his second model, we shall review the criticisms and the subsequent modifications of his model I. In this connection, we shall critically compare Levi's notion of epistemic utility with Hempel's so as to shed light on some unique features of Levi's measure of information.

CRITICISM OF LEVI'S MODEL I

Even though Levi explicitly acknowledges that his model is devised only for a relatively modest task, it is not without its problems. Kyburg, Hacking, Hilpinen and others have raised various objections to it. For our purposes, we shall select those that are directly relevant to our task.

Henry Kyburg complains that "the selection of a number q is a subjective matter.", [Kyburg 1970, p. 188] As an objection, such complaint apparently carries little weight. Though Kyburg does not expand on this point, it is nevertheless not difficult to construct an argument out of it. Presumably, the argument would be something like this:

In epistemological matters such as the acceptance (rejection) of sentences, the constraints on such acceptance (rejection) should be rational and objective ---applicable to all persons in all contexts. The index q surely in a sense is one such constraint, hence it should be objective. Levi's concept of q is subjective, therefore, it violates our epistemological proviso.

Before we take up this argument, let us be clear that the charge that the index q is subjective refers not to the concept of q itself, but to the assignment of values to q . As suggested by Levi, index q is purported to represent the degree of caution an agent has with regard to accepting hypotheses. The assignment of different values of q to an agent (agents) is intended to reflect the different amount of caution an agent (agents) exercised in that regard. Indeed, insofar as the index is so construed, there is an inevitable sense in which the choice of the value of q is a subjective matter. It is subjective exactly in the sense that not only different agents may have different degrees of caution, but the same agent in different contexts of inquiry may also exercise different amount of caution in the acceptance (rejection) of hypotheses (sentences). This however seems to be a reasonable assumption to make in this connection. Hence, even if index q is subjective in this

sense, it is not objectionable at all.

Though the sense in which q is understood to be subjective is tolerable here, it does not mean that Levi's account of the index q is a satisfactory one. In GWT, Levi likens his index of q to Neyman-Pearson's significance level in hypotheses testing. Though the determination of the level of significance, like that of index q , is in a sense unavoidably subjective, the determination of significance level is by no means entirely arbitrary in the sense that the agent can assign whatever values he likes. On the contrary, such determination has a close connection with the problem, especially the practical problem involved. Thus, in a sense, it is constrained by the latter. William Kruskal explains this well:

"Probably the most common significant levels are .05 and .01, and tables of critical values are generally for these levels. But special circumstances may dictate tighter or looser levels. In evaluating the safety of a drug to be used on human beings, one might impose a significance level of .001. In exploratory work, it might be quite reasonable to use levels of .10 or .15 in order to increase power. What is of central importance is to know what one is doing..." [Kruskal 1978, p. 952]

In light of this observation, if index q , as maintained by Levi, is analogous to significance level, then its determination should likewise be closely connected to the the practical problem to which the cognitive problem is directly related. Obviously, due to Levi's cognitivist's predilection, this issue is insufficiently attended to.

Even though the q -index is supposed to reflect the agent's degree of caution, does it mean that q literally reflect true feeling of the agent? The answer to this question is that it does not. Levi's intention is not to use index q as a descriptive index. In fact, he readily admits that it is highly difficult to ascertain whether a definite number of q really represents the agent's "true feelings." Moreover, he also concedes that it is not easy to know with certainty how to assign numbers to one's state of cautiousness. Nevertheless, he maintains that these difficulties seem surpassable if q is not treated descriptively. Indeed, instead of using q as a descriptive index, it is seen as a normative indicator of the following sort. Suppose that different agents have different index q of their own, they then compare conclusions arrived at relative to the different values of q to which they subscribed. They then adjust their values of q in order to obtain the conclusions they wish to reach. Meantime, such conclusions are derivable from values of q within a permitted interval. In this way, Levi thinks that the

choice of a q-index is nothing but "a commitment to having one's inference publicly evaluated in a certain way." [GWT, p. 90] Once again, it is perhaps in this sense, after all, that Levi likens his q-index to significance level in Neyman-Pearson hypotheses testing.

Another difficulty of Levi's model, according to many critics, has to do with his notion of ultimate partitions. Kyburg argues that due to a lack of a criterion for determining ultimate partitions, one is free to accept anything one likes:

"...by an ingenious construction of ultimate partitions, one might be able to arrange for any proposition whatsoever, with nonzero probability, to be accepted." [Kyburg, Ibid. p. 189]

Moreover, an investigator, Kyburg claims, may with a given evidence at the same time (though not in relation to the same ultimate partition) accept both a proposition and its negation. [Ibid. pp. 189-190] Incidentally, criticisms very similar to this are also voiced by Hacking [1967] and Hilpinen [1968]. Both charge that Levi's rule (A) is too sensitive to the choice of ultimate partitions so much so that consequences envisaged by Kyburg are generated. Let us illustrate this with an example of a horse race forecasting: Suppose person A is asked to predict whether horses X, Y, Z,

will win. The forecast is expressed in terms of three sentences: $h-1$: "X will win," $h-2$: "Y will win," and $h-3$: "Z will win." Let the posterior probability of these three sentences, on the available evidence, be $p(h-1,e)=.44$, $p(h-2,e)=p(h-3,e)=.28$. Suppose the question for A now is whether X will win or not. Accordingly, A will have a 2-fold partition---"X will win," and "X will not win." Assume that A has a rather high q -index, say $q > 2/3$, then using rule (A), A will accept that X will not win. On the other hand, if the question is then changed to one which asks which of the three horses will win, A will correspondingly have a 3-fold partition: "X will win," "Y will win," and "Z will win". With respect to this partition, the prediction is, as expected, that X will win. This example clearly illustrates that a hypothesis is accepted relative to one ultimate partition but rejected relative to another. This, the critics contend, warrants a modification of Levi's system. [Hilpinen, pp. 100-101]

Levi's response to this criticism is by invoking the distinction between mere acceptance (acceptance as true) and acceptance as evidence. According to Levi, when a sentence is accepted as evidence, it is regarded as necessarily and certainly true. In contrast, if the sentence is merely accepted, then it remains less than certain and is possibly false. [AR, p. 35] The charge of sensitivity of rule (A) to ultimate partitions would have force, Levi argues, only

if acceptability relative to ultimate partitions were regarded as sufficient for acceptance as evidence. The latter, Levi maintains, is clearly not his position. As a matter of fact, Levi explicitly denies such a position in GWT [pp. 28-29, 149-152] Levi's point here seems to be this. To the extent that rule (A) only legislates mere acceptance, the sentences accepted will at best be true. That is, they should be viewed as "less than certain and possibly false." Thus, even though h-1 is accepted relative to U and rejected relative to U', it creates no real difficulty so long as h-1 remains less than certain. The acceptance of h-1 constitutes a real problem only if h-1 is viewed as certainly true. Clearly, we cannot accept a certainly true sentence relative to U and then reject it relative to U'. Nevertheless, if mere acceptance is what the critics have in mind, then I think Levi's response is sound.

Mere acceptance, for Levi, has to be relativized to the choice of an ultimate partition. Hence, there should be no surprise that different ultimate partitions should yield different acceptances. This is clearly stated by Levi,

"...the conclusion warranted by rule A are conclusions warranted in order to gratify a specific demand for information. Relative to a different demand, it is to be expected that a

conflicting conclusion is to be expected. It is a virtue---not a defect---of rule A that it mirrors this feature." [AR, p. 62-3]

Mere acceptance, after all, is relative to a specific question, and hence relative to a specific ultimate partition. Acceptance as evidence, in contrast, is relativized to a wider scope. As Levi points out:

"Acceptance as evidence via inductive expansion relative to X's problem situation---i.e. the set of questions he recognizes to be serious at the time and system of potential answers he has identified for those problems." [AR, p. 35]

Referring back to Kyburg's earlier charge that the lack of a general criterion for determining ultimate partitions would generate undesirable results, Levi's answers presumably would be this: Demanding such criterion is like demanding a logic of questions and answers that is applicable to all contexts of inquiries and deliberations. Though one cannot categorically deny the possibility of such a criterion, it is highly doubtful, given the highly pragmatic nature of cognitive inquiry, that it is likely to be obtainable. Therefore, I shall leave this as an open question.

LEVI'S PRAGMATIC NOTION OF INFORMATION

In this section, we shall focus on some distinctive features of Levi's model, especially his measure of the utility of information. In "Information and Inference", Levi tries to argue against two concepts of information--one presumably held by himself previously, the other by a majority of writers. The first one is the semantic concept of information which in effect states that the information conveyed by a hypothesis is the semantic relation between the evidence and the hypothesis. This idea can be explicitly expressed as follows:

(I): Let G be an element of M_0 . The information obtained relevant to the question under consideration by accepting G as strongest via induction from $b \& c$ is a semantic property of G or a semantic relation holding between G and $b \& e$. [p. 372]

Contrary to what is asserted in (I), Levi maintains that the information considered here should be treated pragmatically and not semantically. That is, the information demanded by a question depends upon what the question demands.

The second idea of information which Levi wants to reject is the familiar one originally proposed by Popper. It is the notion which is purported to claim that an informative hypothesis is an improbable hypothesis. Again

the idea can be succinctly expressed as follows:

(II): Let $p(G,e)$ be the conditional probability of G given $b \& e$ and let $p(G)$ be the conditional probability of G given b . The information conveyed by accepting G as strongest via induction from $b \& e$ either increases as $p(G,e)$ decreases or as $p(G)$ decreases. [*Ibid.*]

According to Levi, (II) just cannot be right with regard to the information demanded by a question.

Even though Levi's approach to scientific inquiry, as presented in *GWT*, is primarily pragmatic in nature, it is however not until "Information and inference" that he intends to give a thorough pragmatic construal of information. Indeed, it is this pragmatic conception of information that seems to give his model a very unique character. Moreover, it is precisely this pragmatic turn that provides the basis for the rejection of (I) and (II).

For our purposes, we shall use an example, suggested by Levi himself, to demonstrate why Levi wants to abandon (I) and (II). Our strategy here is simply to show how the two notions cannot work in cases where the information demanded is relative to a question. Suppose we have a 1000-ticket lottery, in which the probability of ticket i will be drawn is the same for each i ticket. If our question is to predict whether ticket 1 will be drawn or not, we will have

a 2-fold partition consisting of two sentences: h_1 : "ticket 1 will be drawn" and $\neg h_1$: "ticket 1 will not be drawn." Here, each of these sentences will, given the demand of the question, be as informative as the other. In this case, clearly $m(h, e)$ is not equal to $p(h, e)$. However, if the question is rephrased as to ask "which of the 1000 tickets will win?", the ultimate partition U_e will consist of 1000 sentences of the form: "Ticket i will be drawn." Now each sentence of U_e is, given the demand of the question, as informative as any other. Unlike the previous case, $m(h, e)$ is now equal to $p(h, e)$.

Again, suppose that we have another case similar to the one just mentioned, but this time we know that the lottery has been arranged in favour of ticket 1, for example. Let "Ticket 1 will be drawn" has a probability of .4 and the remaining probability will be divided equally among the remaining 999 sentences of U_e . Given this information, "Ticket 1 will be drawn" presumably will be accepted. But here $m(h, e)$ is not equal to $p(h, e)$, because each sentence of U_e will still have the same m -value while they do not have the same p -value.

(II) demands that $m(h, e) = p(h, e)$ in every situation. We have just shown that this is not generally true, though it might hold in some cases (e.g. case 2). Therefore, (II) has to be rejected.¹¹ Meanwhile, our example also has nicely demonstrated how the demand of a question affects the

information content of an answer. Levi summarizes it well:

"Relative to the question 'which of the 1000 tickets will be drawn?', 'ticket 1 will be drawn' is much more informative than its contradictory. Relative to the question 'Will ticket 1 be drawn?', these two hypotheses are equally informative. Thus the information conveyed by a hypothesis cannot be taken to be semantic property of hypothesis in relation to the evidence." [II, p. 383]

Notice that model I is originally constructed for the relatively simple task of making estimation, prediction and generalization. It might not be equally applicable in other contexts. For example, in choosing among alternative theories or potential explanations, not only does the object of acceptance become more complicated, it is also difficult to determine the ultimate partitions. Levi readily acknowledges this. Moreover, in theory choice, he concedes that "not only is it difficult to determine in any unique way an ultimate partition, but the elements of the ultimate partition will not be equally informative." [II, p. 385]

The second claim incidentally represents a refinement of Levi's original position with regard to the assignment of information to the elements of U. Earlier, Levi attributed

equal information to elements of U via a regular measure function and the elements of U are simple sentences. In theory choice, however, instead of being simple sentences, the elements of U would be sets of sentences with perhaps complex sentences as their components. In situations like this, it is surely not unreasonable to regard them as having unequal information, among other things. This, I think, is presumably the idea behind Levi's second claim.

With regard to the determination of ultimate partitions in theory choice, Levi recognizes that it is not always possible to devise an exhaustive list of potential answers. In order to ensure exhaustiveness, he suggests that a "residual hypothesis" asserting in effect that all other elements of the ultimate partition are false, has to be introduced. In addition, other epistemic factors, like simplicity, explanatory power, and depth etc. may very well enter into the consideration as well. Undoubtedly, these factors would presumably contribute to "a lack of uniformity in the informational values attributed to the ultimate partition." [Ibid.]

HEMPEL'S CONCEPT OF EPISTEMIC UTILITY

Hempel probably was the first one to apply decision theory to cognitive problems. As mentioned before, he sees a close resemblance between practical decision making and

cognitive decision making. Yet there is an important difference, viz., we require a new kind of utilities to replace practical utilities in epistemic decisions. Hempel subsequently coined the term "epistemic utility" to refer to this new kind of utility. Epistemic utilities are supposed to represent the epistemic values or goals that are unique to science. Hempel identifies truth and information as the two salient goals of scientific inquiry. Scientists, according to Hempel, have to maximize these two goals. In the language of epistemic decision theory, this is tantamount to say that scientists' goal is the maximization of epistemic utilities. Our task in this section, as mentioned before, is to review Hempel's notion of epistemic utility and to critically compare it with Levi's notion. By such comparison, one important difference concerning the epistemic preference between Hempel's and Levi's theory will be clearly exposed.

Hempel's concept of epistemic utility is as follows. Let K be the corpus of knowledge, k be a sentence which is both logically implied by K and logically implies K . We say that k has the same information content as K . The common content of h , an hypothesis to be accepted, and k is expressed by hvk . Since $h \equiv (hvk) \& (hv-k)$ [because $h \equiv hv(k\&-k)$], the content of h which goes beyond the information contained in k is expressed by $hv-k$. What is needed here is a measure of the content expressed by $hv-k$.

Following Carnap and Bar-Hillel, Hempel proposes a content-measure in this regard. The content-measure m , as conceived by Hempel, is a function defined over the sentences of a formalized language L , which assigns to every sentence in L , a real number $m(s)$ which satisfies the following conditions: [DN, 154]

- (1) $0 \leq m(s) \leq 1$, for every sentence in L .
- (2) $m(s)=0$ if and only if s is a logical truth of L .¹²
- (3) $m(s_1 \& s_2) = m(s_1) + m(s_2)$, if s_1 vs s_2 is a logical truth

If conditions (1)-(3) are satisfied, then it can be readily seen that m should also satisfy the following conditions:

- (4) $m(s) = 1 - m(-s)$.
- (5) if s_1 implies s_2 , then $m(s_1) > m(s_2)$.
- (6) if $s_1 \equiv s_2$, then $m(s_1) = m(s_2)$

Basing on this characterization of the m -function, Hempel proposes his "tentative" measure of epistemic utility as follows:

"The epistemic utility of accepting a hypothesis h into the set K of previously accepted scientific statements is $m(hv-k)$ if h is true, and $-m(hv-k)$ if h is false; the utility of leaving h in

suspense, and thus leaving K unchanged, is 0."

{Ibid.}

Hence, the utility of correctly accepting h and the utility of erroneously accepted h are expressed respectively by m -function as follows:

$$\begin{aligned} m(hv-k) & \quad , \text{ if } h \text{ is true,} \\ -m(hv-k) & \quad , \text{ if } h \text{ is false.} \end{aligned}$$

Elsewhere, Hempel recognizes that the principle of diminishing marginal utility, a well-known utility principle in economics, is also operative in the cognitive domain.¹³ Accordingly, he tries to reconstruct his concept of epistemic utility in accordance with this principle. The epistemic utility of adding h to k , Hempel maintains, is directly proportional to the amount of new information conveyed by h if h is true, or to the negative value of that amount if h is false; and inversely proportional to the amount of information already contained in k . The modified epistemic measure of information is as follows:

$$\begin{aligned} (a): \quad [a \cdot m(hv-k)]/m(k) & \quad , \text{ if } h \text{ is true} \\ [-a \cdot m(hv-k)]/m(k) & \quad , \text{ if } h \text{ is false,} \end{aligned}$$

where a is a positive constant.

In a nutshell, this is Hempel's concept of epistemic

utility.

CRITICAL OBSERVATIONS ON HEMPEL'S EPISTEMIC UTILITY

When Hempel's utility concept is being compared with Levi's, some of the noticeable differences seem to be these: First, Hempel's content measure function is relativized to the language L ---it is defined over the sentences of L . Levi's content measure, in contrast, is relativized to ultimate partitions---it is defined over the elements of the ultimate partition. For Levi, the m -function is uniform---it assigns equal information content to every element in the partition; but this is not necessarily the case for Hempel's m -function. There may be more interesting differences or similarities between the two concepts, nevertheless, we think these points suffice for this section.

Let us go to examine some of the alleged difficulties of Hempel's notion. The first difficulty, raised by Hilpinen, concerns the fact that Hempel's concept would generate an unacceptable rule of acceptance. Hilpinen's argument is as follows: [Hilpinen, p. 93] Let the epistemic utility of correctly accepting h given e (e here is equivalent to k in Hempel's formula) be $u(h, t, e)$, the epistemic utility of erroneously accepting h be $u(h, f, e)$. Then using Hempel's formulae (A), we have:

$$(1) u(h,t,e) = [k.m(hv-e)] / m(e)$$

$$(2) u(h,f,e) = [-k.m(hv-e)] / m(e), \text{ where } k \text{ is a positive constant}$$

$$(3) u(S,e) = 0 \text{ where } S \text{ is suspending of judgement.}$$

From (1) and (2), we get

$$u(h,f,e) = -u(h,t,e).$$

The expected epistemic utility of accepting h given e is

$$(4) E(h,e) = p(h,e)u(h,t,e) - (1-p(h,e))u(h,t,e) \\ = u(h,t,e)(2p(h,e) - 1)$$

It is obvious that (4) together with (3) yield

$$(5) E(h,e) \geq E(S,e) \text{ if and only if } p(h,e) \geq .5 \\ \text{if } p(h,e) = .5, E(h,e) = E(S,e)$$

Hempel, on the basis of (5), proposes the following acceptance rule:

(H) Accept or reject h , given e , according as $p(h,e) > .5$ or $p(h,e) < .5$, when $p(h,e) = .5$, h may be accepted or rejected, or left in suspense. [DN, p. 155]

Clearly, Hempel's rule is a kind of threshold rule which may

also be called a high probability rule. It is not a satisfactory rule because it immediately leads to the lottery paradox. The problem of Hempel's definition of epistemic utility seems to be this: in the course of getting expected utility, the measures of information cancel themselves out. The result is that the acceptance rule is a purely probabilistic rule. And the information content of a hypothesis has no part to play in its own acceptability. This absurdity, according to Hilpinen, surely warrants a revision of Hempel's concept.

Another alleged difficulty of Hempel's concept of epistemic utility is raised by Levi. Indeed, Levi's criticism of Hempel's concept is particularly interesting here because it helps to bring out a significant difference of preference ordering between himself and Hempel which may otherwise be left less noticeable.

Using Hempel's definition, Levi observes that the utility of accepting false answers is a decreasing function of their utilities of information, i.e.

$$(a) \quad u(h, f) - u(g, f) = -b(C(h, x) - C(g, x))$$

According to Levi, (a) violates condition (D1.b) which requires that the utility of false answers be an increasing function of their information:

$$u(H,f) - u(g,f) = b_f (C(h,x) - C(g,x)), b_f > 0$$

In view of this, Levi concludes: [I&I, p. 380]

"...an investigator who is interested in true and informative answers, but who is constrained to choose among answers guaranteed to be false, should, according to Hempel, minimize the informativeness of the answer he chooses. This in itself seems counterintuitive enough to warrant revising Hempel's proposal."

Let us examine whether Levi's argument is sound. It seems that by pointing out that (a) violates Levi's own condition (D.1b) as the ground for the inadequacy of Hempel's notion is simply not enough. Because one may legitimately question the adequacy of (D.1b) itself. Levi's own condition is indeed an assumption about the preference ordering of false answers relative to their respective informativeness. It in effect states that informative errors are preferable to less informative ones. In contrast, Hempel's (a) maintains in effect that in choosing among false answers, less informative ones are preferable to more informative ones. Now clearly what we have here are two contradictory preference orderings of errors relative to informativeness. So, instead of saying, as Levi does,

that (a) violates (Dl.b), it seems more fair to say that we have two incompatible preference orderings. Therefore, without giving an independent justification of (Dl.b), Levi's argument against Hempel's concept is far from decisive.

Interestingly, if we look more closely into the issue, the conflict between Hempel's (a) and Levi's (Dl.b) is indeed a result of a difference in the assumptions concerning, something like the utility independence of truth and information. In ranking answers with respect to information, Levi presumably has an independence assumption to the effect that informative answers are preferable to less informative ones regardless of the truth values of the answers. In other words, information is assumed to be utility independent of truth.¹⁴ Hempel, on the other hand, seems to assume only a one-way utility independence between truth and information. In the choice of correct answers, informative answers are preferable to less informative ones. However, should we say that this still holds in the choice of errors? Hempel's answer is that it need not be so. For him, a more informative error may mean that it is a bigger error, and therefore there is no reason that bigger errors should be preferable to smaller ones (if less informative errors are construed as smaller ones). Given such interpretation, Hempel may certainly with good reason hold the opposite of what (Dl.b) claims. That is, in the choice

of false answers, contrary to the choice of true answers, less informative answers are preferable to more informative ones.¹⁵ Let us summarize the difference between Hempel's and Levi's preference as follows:

Preference ordering between			
true answers		false answers	
Levi	inf > less inf	inf > less inf	
Hempel	inf > less inf	inf < less inf*	

* inf=informative

FIG.C A COMPARISON OF HEMPEL'S AND LEVI'S PREFERENCES

In light of the above considerations, Levi's criticism of Hempel's proposal needs to be strengthened. However, since Levi just takes (D1.b) as an assumption and apparently gives no independent justification for it, it is safe to say that the violation of (D1.b) is not strong enough to reject Hempel's (a). As we have already mentioned, the confrontation between Levi's and Hempel's assumptions nonetheless helps to illuminate the utility independence assumption in Levi's system.¹⁶

LEVI'S CORPUS REVISION MODEL (CRM)

Ever since "Acceptance Revisited," Levi has been developing a rather elaborate and comprehensive theory of epistemic decisions. Since the model concerns primarily the improvement or refinement of the knowledge corpus, I shall refer to it as the Corpus Revision Model (CRM). CRM, as it were, is not an entirely new and separate system which has no relation whatsoever with the previous model developed in GWT. On the contrary, CRM assimilates most of the materials of the previous model and indeed is an extension of it. Recall that the function of model I is estimation, prediction and generalization; in the new model, as we shall see, all these functions are treated as some special aspects of knowledge revision. Hence, model I shall be seen as a submodel of CRM.

The central problem of CRM, as its name probably suggests, is the rationality of the revisions of the knowledge corpus. Questions like: Why should we revise our knowledge? What kind of revision of knowledge is it? What, if any, is the justification for doing so? are the ones we shall be discussing in the following.

KNOWLEDGE CORPUS K AND LANGUAGE HIERARCHY

Since our problem here is the revision of knowledge, it is profitable to have a clear idea of what its structure and functions are, as seen by Levi. Let us begin by examining its structure. What Levi takes as the knowledge corpus $K_{x,t}$ of an agent X at time t is basically the set K alluded to in earlier discussions. However, for a clear presentation, it is worth restating those conditions which K has to satisfy. (In what follows, I shall refer to $K_{x,t}$ simply as K for convenience.) Corpus K , as it were, is a set of sentences expressible in a suitably regimented language L satisfying the following conditions: (1) K is deductively closed; (2) K contains every items in the deductively closed set UK (the 'urcorpus') consisting of all logical truths, set-theoretic truths, and mathematical truths and whatever counts as expressible in L ; (3) K is consistent. Corpus K not only presupposes a language L , it also requires a meta-language L' to express meta-statements like " h is true in L ", for example. Correspondingly, we may have another corpus K' which contains statements expressible only in L' and statements of L . Some logical relationships obtaining between K and K' would be:

(1) If h is expressible in L , then h is a member of K' if and only if h is a member of K .

(2) " h is true in L " belongs to K' if and only if h

belongs to K.

(3) "h is true in L" belongs to K' if and only if "h belongs to K" belongs to K'. [AR, p. 21]

Analogously, a hierarchy of languages L_1, L_2, \dots and their corresponding corpora K_1, K_2, \dots with similar logical relationships should be allowed. Changes in the corpus K will, Levi maintains, create changes in the hierarchy of the K's in some definite manner. [AR, p. 22] For most purposes, Levi concedes that K is quite enough.

Structurally, the knowledge corpus consists of K and UK. UK itself contains only logical, set-theoretic and mathematical truths. K, which contains UK as a proper subset, contains, in addition to every truth of UK, also empirical sentences consisting of both observation and theoretical sorts. As to the function of K, Levi regards K as a resource for cognitive inquiries and deliberations. Specifically, K is viewed as the standard for serious possibility. With regard to the epistemological status of the sentences belonging to K, they are treated on a par with each other. As these have been discussed earlier, they will not be repeated here. Let us examine the details of CRM.

KNOWLEDGE REVISION---CORPUS EXPANSION

According to Levi, every change in our body of knowledge can be viewed as a change of the corpus K . That is, the change in knowledge is interpreted here as a change from one deductively closed and consistent set K to another set K' . Corpus revision, as it were, is composed of two basic kinds---expansion and contraction. Though it is not uncommon in science as well as in everyday life that replacement of corpus can happen; replacement, Levi claims, is analysable in terms of expansions and contractions. He says:

"Every shift from K to K' is either an expansion, contraction or decomposable into a sequence of expansions and contractions." [AR, p. 24]

Expansion is a kind of revision whereby K shifts to K' which contains K as a proper subset. In corpus expansion, therefore, new items are admitted into the corpus. In contrast, contraction is a kind of revision in which items are removed from the initial corpus. Here one shifts from K to K' which is a proper subset of K .

If changes in the body of knowledge are really analysable into these two major kinds of revision, then the understanding of the condition under which these two kinds of revision are justified, will presumably enable us to

understand the conditions under which changes in knowledge, in general, are justified. By the same token, the rationality of knowledge change hopefully will be illuminated by the apprehension of the rationality of corpus expansion and contraction. Let us go into the details of this subject.

There are two kinds of expansion: routine expansion and expansion via deliberate decision. The first kind includes common observations, direct stimulus responses to sensory stimulation and accepting testimonies from others. Notwithstanding that it is a natural way to expand our knowledge, routine expansion nonetheless might result in adding contradictory information into our corpus. Levi calls this "erroneous expansion"---expansion which results in an inconsistent corpus. Relative to the aim of obtaining error-free information, erroneous expansion surely has to be avoided. Though errors might often be admitted during corpus expansion, one should not be discouraged from expanding the corpus in order to obtain information. On the other hand, though routine expansion is not a perfectly reliable method to get information, it is still a useful way to gather information. After all, as fallible beings, we simply do not have any infallible method in this regard.

Fallible though routine expansion may be, once the results of it are admitted into X's corpus, they should be, from X's point of view, treated as infallibly and certainly

true in subsequent inquiries and deliberations. Levi remarks:

"...once an item is incorporated into a corpus, its pedigree becomes irrelevant to its uses in subsequent inquiries and deliberations." [AR, p. 26-27]

The pedigree alluded to here presumably refers to the source of the information or the way the information is required. Moreover, Levi claims that this applies not only to knowledge obtained via routine expansion but also to that acquired via deliberate expansion as well.

In routine expansion, Levi maintains that X usually takes such routine as reliable and hence demands no justification of the results of such expansion. He says:

"...as long as X assumes that a given routine for expansion is reliable...there can be no question of justifying the result of expansion via that routine ." [AR, p. 27]

Unlike routine expansion, however, expansion via deliberate decision requires justification. The problem situation here is, for the most part, basically the same as that envisaged in model I: X has a problem under

investigation, and a set of potential answers relative to that problem, his objective is to choose one among these potential answers satisfying some cognitive desiderata. Here X has to compare different answers in terms of their respective expected epistemic utilities. He has to choose the best answer in accordance with some specified rational rules. Indeed, it is relative to these rational rules that Levi claims that deliberate expansion requires justification.

The other difference is that routine expansion can lead to an inconsistent corpus whereas deliberate expansion cannot.¹⁷ This is because no one will deliberately choose an answer which will make K into an inconsistent corpus.¹⁸

CORPUS CONTRACTION AND REPLACEMENT

In contracting a corpus, items are removed from the corpus which from X's point of view are certainly and infallibly true. With regard to the aim of gaining error-free information, such an undertaking seems objectionable. However, there are two reasons why X has to contract his corpus.

First, contraction has to be executed whenever a contradiction or inconsistency is detected within the corpus. Such inconsistency may have been generated by

routine expansion. Since an inconsistent corpus cannot serve as a standard for serious possibility, the corpus has to be contracted in order to restore consistency, even though doing so would certainly mean the loss of information. Analogous to deliberate expansion, X presumably has a number of ways to contract his corpus. The problem he faces is to choose a contracting strategy which would minimize the loss of information, among other things. When an item is removed from the corpus, it is no longer viewed as certainly and infallibly true as it once was, but only as a hypothesis. By treating an item as a hypothesis means that it is treated as possibly false, to use Levi's terminology.

Sometimes X will contract his corpus K even though he detects no inconsistency within it. However, the following situation may make X think it is worth contracting his corpus. Suppose p is an item in K and is regarded by X as infallibly and certainly true. Let q , which is not an item in K, be inconsistent with p . Then, according to Levi, from X's point of view, he would be committed to regard q as certainly false. Nevertheless, there may be some interesting features of q , say, explanatory power, which may make it worthy for acceptance into K. But directly accepting q into K would immediately result in an inconsistent corpus. The reasonable thing to do, according to Levi, is to contract the corpus first so that p is

removed from K .¹⁹ Once p is removed from the corpus, it will cease to be certainly and infallibly true but become a hypothesis. Meantime, q will also cease to be certainly and infallibly false but become a possibly true hypothesis. This is the situation where contraction is called for so that X can give an alternative hypothesis a new hearing.

In addition to expansion and contraction, there is replacement of items which are originally in K by those that are not. According to Levi, replacement is analysable in terms of expansion and contraction. Replacement takes place where X wants to shift a corpus K containing h via contraction to a corpus K' which no longer contains h and relative to which both h and its contrary h' are competing hypothesis. X then shift K' via expansion to K'' by admitting h' into K'' .

Initially, X contracts his corpus at t_1 in order to give h' a hearing. After the contraction, both his corpus and his point of view change. From X 's view point at t_2 , his proximate goal is to obtain error-free information. So he shifts his corpus K' to K'' in order to satisfy his goal prescribed at t_2 . That is, at different stages of the replacement process X may have different proximate goals. For Levi, this is quite natural. He says:

"There is no single objective which is the ultimate aim of inquiry. Moreover, there are as

many distinct proximate aims as there are distinct demands for the modification of bodies of knowledge. [AR, p. 32]

Despite the multiplicity of proximate goals in scientific inquiry, they should be treated as special cases of seeking to acquire error-free information. [Ibid.]

RATIONALITY OF EXPANSION AND CONTRACTION

Rationality problems seems to go hand in hand with justification problems. In this particular context, the justification problem, and thus the rationality problem arise exactly when we face a choice situation. Here we have to make comparisons among alternative choices, and finally the optimal choice, in whatever sense it is defined, has to be decided upon in accordance with some criteria. These criteria, if any is possible at all, do serve in a sense as the rationality conditions of the choices. Against this backdrop, let us examine what the rationality conditions of CRM are.

The rationality condition for expansion via deliberate decision evidentially is expressed by the familiar rule (A) presented in GWT. For our convenience, it is restated as follows:

Given K , a finite U , information-determining probability function M defined over the Boolean algebra of elements of U , expectation-determining probability function Q defined over the same algebra, and an index of caution q , X should reject all and only those elements of U satisfying $Q(h_i) < qM(h_i)$. [EOK, p. 53].

Notice that the above condition presupposes that there are no rival problems with conflicting demands under consideration.²⁰ Moreover, the condition proposed is heavily context-dependent---it depends on X 's corpus and credal state (i.e. his judgement of credal probability), his demand for information, his ultimate partitions, his evaluations of information value of the potential answers and his index of caution q . [EOK, p. 56] In other words, rule (A) is a paradigmatic rationality rule of local induction. Moreover, it is also primarily a epistemic version of the Bayesian rule of maximizing expected utility.

Is the same Bayesian principle operative in corpus contraction as well? As we all know, X contracts his corpus when he detects an inconsistency in his corpus or he wants to reconsider a rival hypothesis. Since items are removed from the corpus in contraction, it means that informative items are removed from the corpus. Then, the major concern for X in contraction is the loss of information. The

problem of truth is clearly not the concern here simply because there is no apparent risk of importing errors into the corpus in contraction. If this is so, then the problem X has is probably the minimization of information loss, among other things. Theoretically, a minimizing rule of some sort is needed.

The problem, though, is not as straightforward as it might look. Intuitively, a contraction (or contraction strategy) which best minimizes the loss of information should be the most preferable one. This, however, requires some ways of measuring the information values of the items of the corpus. But not every item in the corpus is expected to have the same information value. It is natural to assume, therefore, that some items are more informative than others, no matter what measures of information are used. That is, even if we could successfully delimit a set of potential answers (i.e. potential items to be removed), and even assuming that the set is fairly small, there is no reason to expect that they all have the same information value. Hence, the problem of measurement still remains. (Apparently, Levi's α -function seems to be of no use here because it only attributes information to elements of ultimate partitions and not to elements of \mathcal{A} .) The difficulty here is an important one. It will be examined in the next chapter.

FOOTNOTES

1. Levi, EOK, p. 22.

2. Levi, AR, p. 35.

3. The definition of deductive closure is as follows: a set S is deductively closed if and only if whenever h and g are members of S , $h \wedge g$ and the deductive consequences of $h \wedge g$ in L are also members of S . [GWT, p. 26]

4. The subscripts e of U , M , S , C refer to new evidence, and can be dropped when total evidence consists only of b .

5. For example, in Levi's I&I.

6. Levi assumes that this function (together with the V -function and C -function) is unique up to a positive linear transformation, i.e. their origin and unit are arbitrary choices. Here, the values of α and 0 are arbitrary choices. Furthermore, Levi simply assumes that these values to be utilities without showing it à la von-Neumann-Morgenstern. On the other hand, these functions could of course be indexed by subscripts relative b or $b \wedge e$. For simplicity sake, we just leave out all the subscripts. This applies to other functions as well.

7. Such a result is obtained by theorem (T.5), which Levi says is derivable from (E) in conjoining with another theorem (T.4). (T.6) states:

$$(1-\alpha)/\alpha < (C(W,x)-C(X,x)) / (T(X,t)-T(W,f))$$

Our calculation reveals that (T.6) should be:

$$(1-\alpha)/\alpha < (T(X,t)-T(W,f)) / (C(W,x)-C(X,x))$$

Proof:

$$V(W,f) < V(X,t) \text{ ---- (T.5)}$$

By using (T.4b) and substituting, we get

$$\alpha T(W,f) + (1-\alpha)C(W,f) < \alpha T(X,t) + (1-\alpha)C(X,x)$$

$$(1-\alpha)(C(W,x)-C(X,x)) < \alpha(T(X,t)-T(W,f))$$

Therefore, the result.

However, even though our result differs from Levi's, the final result is the same, because $(1-\alpha)/\alpha < 1$.

8. In his original version, Levi does not include (5). In his EOK, however, it is included. In fact, (5) is only the familiar multiplication theorem in the probability calculus.

9. The rule for ties states that the element of M_e with a strongly maximal expected epistemic utility is to be accepted. [GWT, p. 84] We need two more definitions to understand the rule: (1) $E(h,e)$ is strongly maximal in M_e if and only if $E(h,e)$ is maximal, and for every G other than H , such that $E(G,e)$ is maximal in M_e , $C(H,e) < C(G,e)$. (2)

$E(H,e)$ is maximal in M_e if and only if, for every G in M_e , $E(G,e) < E(H,e)$. [GWT, p.83]

10. For a complete version, see GWT, p. 86.

11. By abandoning (II), Levi does not imply that $m(h,e)$ is not probability in any sense. He admits that it is probability only in the sense that it obeys the usual formal conditions of probability calculus. What he denies, nevertheless, is the fact that $m(h,e)$ is probability in the sense of fair betting rates or degrees of confirmation in Carnap's sense. [I&I, p. 383]

12. Elsewhere, Hempel gives an alternative formulation of this condition:

$m(s)=1$, if s is a contradiction in L . [II, p. 76]

13. Hempel, II, p. 76.

14. An attribute A is said to be utility independent of another attribute B if the conditional preference of A on B is independent of the values of B . For an alternative definition, see Keeney and Raiffa [1976, p. 226]

15. The idea of this argument was originally suggested to me by Professor Nicholas.

16. To be exact, it is the additive utility independence rather than mere utility independence that Levi assumes. (The former notion is a stronger notion than the latter.) This can readily be seen by Levi's own construction of the utility function:

$$u(h) = \alpha T(h) + (1-\alpha)C(h)$$

which is additive in form. For more detail discussion of this property, see Keeney and Raiffa, 1976, p. 230.

17. Levi, AR, p.28

18. Levi here talks as if acceptance as strongest via induction is sufficient for acceptance into the corpus, this is true only when we have one problem. It seems that more constraints have to be invoked for acceptance into the corpus: (1) the item should be the best answers to lots of questions, (2) the item is not seriously challenged or defeated, (3) the item itself is unproblematic.

19. It can be shown in the next chapter that a competitor can never be preferred to something in K when challenged with respect to some question Q and ultimate partition U .

20. Notice that this is a rule for acceptance as strongest via induction with respect to a question Q and ultimate partition U . cf. n. 18.

CHAPTER VII

EPISTEMIC DECISIONS AND THE DUHEM PROBLEM

Given Levi's epistemic decision model, it is interesting to see how the Duhem problem is represented within CRM. In this chapter, we shall try to reformulate the Duhem problem within the Levian decision-theoretic framework. Recall that the Duhem problem, in its original formulation, states that scientific testing is necessarily ambiguous. There is no rational rule to dictate which component of the theoretical complex is to be rejected relative to an anomalous instance. With the minimal constraint of consistency, scientists can do whatever they see fit for theoretical adjustments.

As a first approximation, the Duhem problem, rephrased in the language of the Corpus Revision Model (CRM), would be something like this: •

Given a corpus K , an observation sentence e is accepted into K through routine expansion (in this case via observation). After e is accepted into K , it is found that it contradicts some items of K , say, the deductive consequence of a subset M of K . Now we have a contradiction within the corpus and an inconsistent corpus results. Since an inconsistent corpus fails to

be a standard for serious possibility, contraction of the corpus is demanded. But if our only goal is to restore consistency, there is a number of ways to contract the corpus and no one way is better than any other.

The interesting question here is to see how the Duhemians would respond to this. Could they, to begin with, endorse the contraction strategy? If so, would they recommend any unique contraction strategy, or just any contraction strategy? For strict Duhemians, due to their commitments to their version of holism, it is simply not possible for them to offer any unique solution to this question. Indeed, if they recommended contraction at all, they would allow any consistency-preserving contracting strategy exactly because they were unable to provide any specific contracting strategy. This position seems to conform with the Duhemian general position---there is, after all, no rational rule conditioned on the anomaly alone governing the revision of knowledge corpus. In a slightly different way, Quinean Duhemians would presumably add simplicity as the only constraint in shrinking the corpus. There is no question that Levi would recommend a contraction strategy in this situation. But the question is: would he allow such minimal constraint to be sufficient for the task?

Levi presumably would allow more constraints than those that would be allowed by the Duhemians. For example, he would probably include utility assignments (to the items of the corpus), among other things, to govern corpus revision. As mentioned before, Levi takes only relevant question in contraction as the question of the loss of information, and claims that we do not have to worry about the question of truth. This is because in contraction, items are removed from rather than admitted into the corpus, so there is no danger of accepting incorrect answers into the corpus. But removing items from the corpus surely means the removal of items that were initially regarded as informative. Correspondingly, if truth is not the issue here, the epistemic utility of truth need not be taken into consideration either.¹ What is pertinent here is clearly the epistemic utility of information. On the other hand, if contraction surely guarantees the loss of information, then the relevant question is how to minimize information loss. Indeed, this is presumably what Levi wants.² The natural thing to do now seems to devise some information loss minimizing rules. If such rules are forthcoming, then we seem to have a ready solution to a Duhem problem.³

QUESTIONS CONCERNING THE FIRST APPROXIMATION OF THE PROBLEM

The problem however is not so straightforward as it looks. There are questions to be answered before the solution is really forthcoming. For example, questions like: Is the utility of information sufficient here? What about the utilities of explanatory power and simplicity? Do they have a role to play in contraction as well? What is to be counted as an anomaly, within the present framework? Is an observation that generates a contradiction within the corpus an anomaly? These are some of the questions that have to be addressed before we can have a clear picture of what the problem is. In what follows, we shall try to answer these questions one by one.

Our decision-theoretic formulation of the Duhem problem assumes that the observation report e is accepted right into the corpus. But it is by no means clear how e is accepted. There are two ways in which e can be accepted---routine expansion and deliberative expansion. According to Levi, observation report e is accepted via routine expansion. Now the observation report e is not any ordinary observation, it is an observation the acceptance of which generates a contradiction within the corpus. The question here is whether any inconsistency-inducing item is to be accepted only via routine expansion. Sometimes, inconsistencies are resulted from deliberate expansion. The reason for this is simple. Though no one would intentionally accept a false

item into the corpus via deliberate expansion, there is no guarantee that such acceptance would not create inconsistency within the corpus. Therefore, insofar as creating inconsistency is concerned, it seems fair to regard both routine expansion and deliberate expansion as responsible for causing inconsistency within the corpus. In other words, an inconsistent corpus should not be seen only as the result of routine expansion.

The above observation immediately opens up once again the question of how observation is obtained through deliberate expansion. That is, in what sense is observation obtained through deliberate expansion? To say that observations are obtained through deliberate expansion requires explicating the sense in which an observation is regarded as an answer to a question. This is because in deliberate expansion what are accepted are only answers to questions. Is there, then, a sense in which observations can meaningfully be regarded as answers to questions?

Francis Bacon used to say that nature is basically mute. In order to understand her, we have to put questions to her. One practical and effective way of asking her questions is to do experiments. Under such a view, experimentation is indeed a way of raising questions to nature. Experimental results---observations, among other things---can likewise be regarded as answers which the experimenter tries to elicit from nature. This Baconian

interpretation of observations is particularly pertinent here. Incidentally, recent discussions in cognitive psychology and philosophy of science have made us more and more aware of the fact that observations in science are never passive but highly active and deliberate cognitive processes. Symbolically, doing a well-planned, carefully executed experiment is very much like asking a well-formulated, clear and relevant question. In such a way, observations resulted from such experiments can meaningfully be seen as answers to questions. If such an interpretation is accepted, then we shall regard observations as obtained through both routine and deliberate expansion. Indeed, by doing so, we are interpreting doing experiments as instances of deliberate expansion, thus extending his original sense of the word.⁴

Another interesting question to be asked here is this: when will an observation be treated as an anomaly? If an observation e is to be accepted via deliberate expansion, it is to be accepted given K. From X's point of view, every item of K is to be treated as certainly and infallibly true. Suppose before e was accepted into the corpus, it was recognized that it would contradict some elements of K. Then from X's point of view, e should be viewed as certainly and infallibly false, hence should not be admitted into K. The failure of so doing would be equivalent to accepting a patent falsehood. Given such a circumstance, would it

follow generally that whatever sentences that are found to be contradicting the elements within the corpus would be immediately regarded as false and be rejected? If this were so, then whatever accepted would become impervious to revision of any sort. Alternatively, it would also not be possible to have any falsifying instances or even competing hypotheses relative to the elements initially accepted. By the same line of reasoning, anomalies of any sort, under such a view, are not possible.

However, some might argue that Levi's system needs not subscribe to such a view. Suppose, the argument goes, that prior to acceptance, X may recognize that e is a contradiction-inducing observation. That is, from X's point of view prior to acceptance, e is regarded as certainly and infallibly false. Still, X may have good reasons to accept e into his corpus. For example, X may find that e is an instance of many occurrences of the same event that demands serious attention. Or, e may be an observation resulted from a well-planned, skillfully executed important experiment. These, among other things, are reasons that make e warrant serious attention and even acceptance. Under such circumstances, X though knows full well that accepting e into his corpus would certainly render his corpus inconsistent, but the price may be perceived by him as worth paying if only e might indirectly contribute to a better corpus.

If e were really regarded as certainly and infallibly false, i.e., $p(e)=0$, according to this argument, would the utilities of e , whatever they might be, be high enough to make e acceptable into the corpus? The answer seems to be No. This can be shown by the following argument.

Let h be in K , g be a competitor for h , $u(C_h, t)$, $u(C_h, f)$ be the epistemic utility functions of h at truth and falsity relative to the problem P , or ultimate partitions U respectively, C_h is the content value for h .

Analogously, we have

$$u(C_g, t), u(C_g, f)$$

Correspondingly, we have their respective expected epistemic utilities as follows:

$$\begin{aligned} EU(h) &= p(h/K)u(C_h, t) + p(-h/K)u(C_h, f) \\ &= u(C_h, t) \quad \text{since } p(h/K)=1 \\ EU(g) &= p(g/K)u(C_g, t) + p(-g/K)u(C_g, f) \\ &= u(C_g, f) \quad \text{since } p(g/K)=0 \end{aligned}$$

We then have

$$EU(h) \succ EU(g) \text{ if and only if } u(C_h, t) > u(C_g, f)$$

That is, $h \succ g$ if and only if $u(C_h, t) > u(C_g, f)$

But according to Levi's postulate (1), correct answers ought to be epistemically preferable to errors, regardless of their respective content. Therefore

$$u(C_h, t) > u(C_g, f)$$

In view of the above argument, if an observation e is regarded as certainly and infallibly false, i.e., $p(e)=0$, no matter how high its utility value may be as an answer to some question or questions, it must not be accepted into the corpus. The problem, however, remains to be this: Are anomalies possible at all within CRM?

If Levi's epistemological infallibilism is seriously intended, it seems that any item inconsistent with or perceived to be inconsistent with K would automatically be given probability zero. In light of our argument, we have already shown that once an item is given probability zero, no matter how valuable it is (in terms of utility), it still cannot be accepted. This seems to be a very rigid form of epistemological conservatism indeed. That is, once an item was accepted, it could never be removed. As a result, disagreement is simply not possible.

Had Levi held such a view, such consequence of course would have been inevitable. However, Levi also allows the possibility of contraction and replacement of the corpus---an accepted item in K can be removed and replaced by an item inconsistent with it. But how is this possible? As we have pointed out before, Levi's epistemological infallibilism is not absolute. As a matter of fact, his position is basically corrigibilistic. K is considered as certainly and infallibly true only when it is considered as a standard for serious possibility at time t . As a standard

for serious possibility, at time t , it is not open to criticism or regarded as false at t , but this by no means implies that it is absolutely immune from criticism and change. In fact, even as a standard, it is by all means open to revision and pervious to change. Therefore, even an item with a low probability can be accepted into the corpus, provided its probability is non-zero. On the other hand, if an item prior to acceptance was perceived to be patently inconsistent with K , it would be given a zero probability and would never be accepted into K . But the question is whether this item would be seen as an anomaly? Within Levi's system, it seems that an anomaly a will occur only after a is accepted into K . That is, a is given probability one. That means that a must be already within the corpus. There is no anomaly outside corpus K . Incidentally, this idea seems to conform well with our commonly held idea of anomaly--- an anomaly is possible only relative to a theory.

But is an observation that contradicts items of the corpus sufficient to make it an anomaly? The answer seems to depend on what kinds of items to which the observation is contradicting. It is clear that not every contradiction within the corpus can be viewed as a result of an anomaly. Conceptual inconsistency is one obvious example. Indeed, it is only when the observation comes into conflict with a theory, or to be more exact, the deductive consequences of a

theory, that we begin to have an anomaly. Due to the fact that Levi gives no clear indication of what he would regard as an anomaly, we would simply assume that such a view is compatible with his system. On the other hand, would there be contradiction between observations? Since it is reasonable to expect that individual observations are logically independent in isolation from theory, such contradictions are not likely.

Lakatos, and Laudan following him, as mentioned before, impose a stricter definition on the notion of anomaly. According to their view, an observation e would not be considered as an anomaly relative to a theory T if there existed no other competing theory T' which was progressively better than T and of which e was not counted as an anomaly. It is not clear whether Levi would entertain such a concept of anomaly. For the sake of argument, however, let us explore this possibility.

Suppose that Levi did subscribe to this interpretation of anomaly. Then within the corpus K , there would exist a progressively better theory T' other than T such that it, or its deductive consequence did not contradict e . If this were the case, then there would be no problem in localizing falsification--- T certainly is the culprit. And the Duhem problem is easily solved. This is because according to the conditions laid down by Lakatos, T is falsified if the following conditions are satisfied: (1) there exists a

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theory T' which is progressively better than T , (2) e contradicts T but not T' , (3) T' but not T explains e . Surely, the satisfaction of these three conditions is tantamount to having a case of falsification. If K did contain T' , then obviously we would have an unambiguous case of T being falsified. However, the crucial question here is how well the notion of a theory being more progressively better than another fits with Levi's model. That is, could K contain T' which is progressively better than T which is already in K ? The answer to this question is that K could not contain T' . Since every item within the corpus is regarded as having equal epistemic status,⁸ it is simply not possible to have theories T' and T coexisting with one another within the same corpus, regardless of how "progressively better" is interpreted.

Presumably the difficulty of fitting this concept of anomaly into CRM arises because theories in the corpus, as seen by Levi, are accepted theories. It means that they are at least accepted as true and ipso facto be treated on a par with each other. Secondly, it is because all items in K already have their "histories" suppressed. If theories were not accepted, then the notion of a progressively better theory T' relative to T is not a totally objectionable concept. As a matter of fact, Lakatos and Laudan require no accepted theory. Neither a theory nor its progressively better counterpart need to be accepted in the first place.

Avoiding the notion of acceptance, it seems that the difficulty encountered by CRM can also be avoided. Nevertheless, this problem may only become a genuine problem when the Lakatosian concept of anomaly is regarded as an adequate concept. If it were not the case, then the fact that it did not fit well with CRM would not affect its plausibility a bit. At this point, I will leave the matter as an open question.

What probability value can be given to e, prior to and after it is accepted into the corpus? As expected, within CRM, when e is accepted into K, it is automatically assigned a probability of unity. But what is the credal probability of e prior to acceptance? There are two cases to be considered here. First, if X did not realize that e would contradict items of K, there would be no reason for X to assign e zero probability. On the other hand, since it is not yet accepted into the corpus, e should not be attributed a probability value of unity either. Under this circumstance, e may be assigned a value between 1 and 0, i.e., $1 > p(e) > 0$. In other words, the credal probability of e prior to acceptance is largely left undetermined. Second, X might know that e might stand in direct conflict with some items of K. In this case, e might be given zero probability. As argued before, once e is given zero probability, no matter how interesting it might be, it could not be accepted into the corpus. But if e did have a

probability greater than zero, but much less than unity, its probability would be increased substantially provided it were interesting enough from X's point of view. Nevertheless, no matter how the probability of e increases, it should not be increased to a value of 1 prior to acceptance. The problem with Levi's system is that there is no probability dynamics of any sort to explain how the probability of e can be shifted from a value of less than 1 to that of unity. It seems that the shift of probability value of e is all accomplished by the very act of acceptance.

As we all know by now, the corpus as a standard for serious possibility has an important role to play in accepting hypotheses. The question here is whether the corpus also has a role to play in the acceptance of observations. At first glance, it seems that K has no apparently relevant role to play with respect to the acceptance of observations. K functions only in differentiating truth-bearing hypotheses that are serious from those that are not, and observations are not proper hypotheses. However, just as we have already argued that observations can be seen as answers to questions, there is no reason to think that they cannot be seen as hypotheses, albeit low-level hypotheses. For example, observation statements like "the ball is red," "this stone is heavy," etc., though expressed as particular statements, can pretty

well be regarded as two low-level hypotheses. If this is so, then the corpus as a standard for serious possibility will also be functional in accepting observation statements. From this it also follows that there will be statements that are serious and statements that are not, though the sense in which they are regarded as serious may be slightly different from that in which proper hypotheses are regarded as serious.

Let us summarize what we have done so far. We have argued that observations can be obtained, in a derivative sense, via deliberate expansion. We have also discussed the way an observation constitutes an anomaly within the corpus. This helps to sharpen the concept of anomaly within Levi's model. In light of these discussions, our first approximation of the Duhem problem is now shown to be too crude to be useful for later investigations. In the next section, we shall try once again to formulate the Duhem problem in light of these considerations. It is hoped that a more adequate representation of the Duhem problem can be provided.

A SECOND APPROXIMATION OF THE DUHEM PROBLEM

Suppose a corpus K (assuming it has three elements, i.e. $K = \{H, A_1, A_2\}$) is confronted with an anomaly, what options does X have in revising his corpus depends largely

on how he conceives the problem. In view of the previous discussion, there is another version of the Duhem problem represented in a slightly different way from the one in the last section. As before, this version of the Duhem problem is also formulated in terms of corpus contraction. However, we need a notion of maximally consistent set proposed by Rescher to formulate the problem. According to Rescher (1973, p. 78), $S' \subseteq S$ is a maximally consistent set (mcs hereafter) if (a) S' is a non-empty subset of S , (b) S' is consistent, (c) no S -element that is a non-member of S' can be added to it without generating an inconsistency (so that for every proposition p in S which is not in S' , the set $S' \cup \{p\}$ is inconsistent.) With the help of this concept, the Duhem problem is represented as follows:

When an anomaly e is admitted into the corpus K , K becomes an inconsistent set $K' = K \cup \{e\} = \phi$. X knows that at least one of the members of K' has to be removed in order to restore consistency. But there are alternative ways of contracting K' thus generating a number of corpora. With the help of these corpora, corresponding mcs's are formed. The Duhem problem is to choose the best mcs relative to some rationality principles.

With respect to the Duhem problem stated above, there are at least six ways to contract the corpus. The contraction

strategy may involve any one of the following ways:

- (a) remove H
- (b) remove A_1
- (c) remove A_2
- (d) remove H and A_1
- (e) remove H and A_2
- (f) remove A_1 and A_2 .

In addition, relative to the corpus $K' = \{H, A_1, A_2, e\}$, there is an additional way to contract, i.e.

- (g) remove e.

(g) is an admissible move because in CRM, once e is accepted, into the corpus, it is treated on a par with other items within the corpus. That is, they all have the same epistemological status in terms of their truth values. Therefore, e, like any other items within the corpus, should not be given any privileged status. Removing e from the corpus hence is a totally legitimate contraction strategy. Meanwhile, choosing (g) is tantamount to leaving the corpus K intact. Relative to corpus K' , we have seven ways of contradicting the corpus. As a matter of fact, different ways of contracting yield different corpora. In this case, the seven ways of contracting the corpus produce seven different corpora. That is, strategy (a) yields a corpus K_a

which contains $C_2 = \{A_1, A_2\}$, strategy (b) generates K_b which contains $C_b = \{H, A_2\}$ and so on.

With the help of the notion of maximally consistent set, we are able to screen out some of the sets that are not maximally consistent. Thus, it enables us to narrow down our choices. In this case, the corpora generated by strategies (d), (e), (f) (i.e. C_d , C_e , C_f), though consistent, are not maximal sets because each of them can still admit an item while remaining consistent. For example, C_d can admit A_1 or H , C_e can admit A_2 or H and so on.

Recall that in contracting a corpus, it is guaranteed that there is a loss of information. The introduction of mcs's is to warrant that each contraction should preserve the maximal information of the set. In this way, the effect of introducing mcs's here is equivalent to the laying down of something like an information-loss minimization rule relative to corpus contraction.

As we have just mentioned, the concept of mcs helps us to narrow down our choices, the question now is how to evaluate the mcs's, or alternatively, their respective contracting strategies. Indeed, in order to determine which mcs (or their corresponding contracting strategies) is the optimal one, we need some ways of measuring the epistemic utility of the individual items or

groups of items to be removed. That is, we have to be able to assign values to $u(H)$, $u(A_i)$, and so on. Following Levi, we would only need the utility function of information for measuring the epistemic utility values of the different items and their conjunctions,⁹ the utility of truth is not needed here. The optimal choice seems to be the strategy whose items to be removed has the least u -value. This means that the choice which involves the minimum loss of information is the optimal choice. Correspondingly, the corpus which is created by such contracting strategy is the optimal corpus. If there is a tie, the two alternatives and hence their respective corpora should be regarded as equally optimal.

CRITICAL COMMENTS ON THIS VERSION OF THE DUHEM PROBLEM

Let us examine some of the merits and weaknesses of this formulation of the Duhem problem and the way to solve it. First, this version seems to be a natural way of utilizing CRM in representing the Duhem problem. It conforms well with the machinery of CRM. The difficulty, however, lies in the assignment of utility values to the items that are to be removed. Even if we confine ourselves to the utility of information, there is no proper utility function within CRM suitable for the task. It is important to note that Levi's measure is only on ultimate partitions

relative to questions and there is no measure on items of K independent of questions. Therefore, unless we have availed ourselves of some such utility function, we simply have no clear way to determine which elements are of lesser information value. Without this determination, we simply cannot determine which contraction strategy is optimal.

Furthermore, there is no explicit way within CRM to reflect the difference between the hypothesis under test (H), and the auxiliary hypotheses A_1 and A_2 . In general, it is reasonable to expect that hypotheses are different from each other in terms of generality, explanatory power, simplicity etc. These differences may somehow contribute to the differences in their respective epistemological status, e.g. degree of corrigibility. For instance, a hypothesis having high generality and great explanatory power ought to be, ceteris paribus, attributed a higher epistemological status than a hypothesis of low generality and explanatory power. Of course, the problem of determining the epistemological status of the various items within the corpus is a notoriously difficult task. But the point here is that hypotheses do differ from each other with regard to these features. Before H and A 's are removed from the corpus K , they are separately assigned probability 1. When they are removed from K , however, their probabilities shift from 1 to a value less than 1 and are treated as bona fide

hypotheses---they are possibly false. In both cases, however, there is no clear indication within CRM that there is any epistemological difference between these two types of hypotheses. They are just treated on a par with each other. Elsewhere, Levi concedes that there are differences in terms of degrees of corrigibility among members of K, yet it is not clear whether such differences would apply to H and A's as well. Furthermore, it is also not clear whether within CRM there is any connection between corrigibility and explanatory power. generality and simplicity.¹⁰ Except for saying that the corrigibility of an item refers to its degree of removability from the corpus, Levi indeed has very little to say on this matter.

In sum, the chief difficulty is : CRM lacks a utility function which is capable of attributing information to items of K which are to be removed. But this may very well be overcome simply by exploiting Levi's own question relative measure of information. That is, in the absence of a global measure of information on items of K, our strategy is to measure K via the local question-answering technique, i.e. to see how K can "offer" local information with respect to questions in a question domain. Indeed, this is basically the idea behind our approach in measuring corpus performance.

CORPUS PERFORMANCE AND PROBLEM SOLVING

There is another way to overcome the above difficulties. Recall that one of the difficulties hinges on the problem of measuring individual items and their conjunctions to be removed. Since that requires a utility function that is capable of attributing utilities to items of K and that CRM simply lack one such function, this creates a roadblock for solving the problem. However, this difficulty can be bypassed by simply avoiding the measurement of items to be removed and this will spare us of the required utility function. The idea of our approach is indeed rather straightforward. Recall that a set of corpora is created by deleting items from K' . Different ways of contracting the corpus K will produce different corpora, i.e., K_a, K_b, \dots . The idea here is to get some kind of measure of these K_i 's in order to evaluate their merits respectively. Or, in the language of the present approach, their respective performance. To be more specific, we shall try to measure how well a corpus performs in sustaining answers to questions.

Before we go into any detail of our approach, let us first clarify the notion of corpus performance intended here. Simply put, the cognitive performance of a knowledge corpus is understood here as the question-answering (problem-solving) capacity of that corpus. Indeed, our idea of corpus performance is a reconstruction of Laudan's idea

of theory performance [1977]. It is then obligatory for us to briefly review Laudan's idea.

For Laudan, the idea of theory performance is closely tied with his idea of problem-solving. Loosely put, how well a theory performs depends on how well it solves scientific problems. Laudan divides scientific problems into two major types---empirical problems and conceptual problems. Empirical problems are problems of facts of the empirical world. Laudan calls them "substantive questions about the objects which constitute the domain of any given science." [1977, p. 15] Conceptual problems, on the other hand, are problems concerning how empirical data are represented, structured and systematized. Laudan refers to them as "higher order questions about the well-foundedness of the conceptual structures (e.g. theories) which have been devised to answer first order questions." [p. 48] The dichotomy of empirical and conceptual problems here is by no means absolute. Quite naturally, Laudan, like many other writers, allows that there are problems which are intermediate between clear-cut empirical problems and conceptual ones.

Of empirical problems, there are three types: (1) unsolved problems---problems which have not yet been solved by any extant theory; (2) solved problems ---those which have been solved; and (3) anomalous problems---those which a particular theory has not solved but which its competitors

have. [p. 17] There are two kinds of conceptual problems as well---internal conceptual problems and external conceptual problems. The former type of problem occurs when a theory contains some inconsistencies, or when its basic concepts are vague and ill-defined. The second type of problem arises when a theory T is in conflict with another theory T' which the proponents of T take to be well-supported. In other words, internal problems are intratheoretic problems which are problems within a given theory. External problems are intertheoretic problems which involve problems between theories. Any external problem, according to Laudan, may be the result of some problems of the following relationship between theories---entailment, reinforcement, compatibility, implausibility and inconsistency. [p. 54]

According to Laudan, the construction of theories is to solve scientific problems. But what exactly do we mean by saying that a theory solves a problem? Does it mean that the theory successfully explains a certain fact or set of facts in the nomological-deductive sense? According to Laudan, the answer to this is a definite No. Laudan maintains that if problems are not facts, then a fortiori, problem solving is not explanation of facts. Laudan says,

"...problems are very different from "facts" (even

"theory laden facts") and solving a problem can

not be reduced to "explaining a fact." " [p. 16]

A problem is conceived as being solved, Laudan maintains when within a particular context of inquiry, the investigators properly no longer treat that particular problem as an unanswered question. [p. 22] More importantly, whether a problem is solved essentially depends on what relation it has with the theory. If a theory entails a statement of the problem, even an approximate one, then that theory is supposed to have solved that particular problem:

"a theory may solve a problem so long as it entails even an approximate statement of the problem..." [p. 22]

In other words, an entailment relationship between a theory and a statement of the problem is the sufficient condition of a problem being regarded as solved. Elsewhere, Laudan expresses the meaning of problem-solving in a different way. He says,

"...any theory T, can be regarded as having solved an empirical problem, if T functions (significantly) in any schema of inference whose conclusion is a statement of the problem."¹²

I take this to mean that when T stands as a sufficient condition of (a statement of) the problem, T is said to solve the problem. Despite Laudan's efforts in explicating the meaning of problem-solving, he still lacks a satisfactorily clear notion of problem-solving. On the other hand, Laudan's other ideas are quite useful for our purposes. For example, his idea of measuring the adequacy of theories is particularly pertinent here.

The Laudanian idea of measuring the adequacy (what we refer to as performance) of a theory is expressed by the following statement:

"The adequacy or effectiveness of individual theories is a function of how many significant empirical problems they solve, and how many important anomalies and conceptual problems they generate." [p. 119]

More specifically, the adequacy of a theory, according to Laudan, is measured by the number and importance of empirical problems it solves minus the number and importance of anomalies and conceptual problems it generates.

Suppose the above idea is amenable to a formal representation. Suppose a theory T solves m empirical problems and generates n conceptual problems within a domain D . Let P_i be the value obtained if an empirical problem P

is solved and w_i be the weight of the problem. (w_i is a real number representing the relative significance of the problem within D); P'_j be the value obtained when a conceptual problem P' is solved and w_j be its corresponding weight. The adequacy or problem-solving effectiveness of T is calculated by means of the following formula:

$$PF(T) = \sum_{i=1}^m w_i P_i - \sum_{j=1}^n w_j P'_j \text{ ---- (g)}$$

Formula (g) is in effect only an oversimplified way of expressing the idea of problem-solving effectiveness of a theory. It assumes that solving empirical problems is the same as solving conceptual ones. As a consequence, it also assumes that the values obtained from solving both kinds of problem are commensurable. Nevertheless, we have reasons not to accept these two assumptions. First, the sense of solving empirical problems, as explicated in the previous paragraph, is in fact quite distinct from the sense of solving conceptual problems. Solving empirical problems requires, among other things, subsuming the problem under an inference schema. It is not at all clear, however, that this notion is applicable to conceptual problems.

If the sense of solving empirical problems is not the same as that of solving conceptual problems, then the values assigned to a solved conceptual problem need not be commensurable with the values assigned to a solved empirical

problem. If this is the case, then formula (g) simply cannot work without proper modifications.

Notice that the notion of problem solving effectiveness encounters these difficulties only when both empirical and conceptual problems are taken into account. If we restricted it to only empirical problems, the difficulties would presumably be avoided. Indeed, our idea of corpus performance will be explicated by exploiting this idea. In other words, the performance of a knowledge corpus is measured only in terms of its problem-solving effectiveness relative to empirical problems.

Even though Laudan's own explication of problem-solving is far from satisfactory, it is interesting to find that it can receive a more precise meaning when properly incorporated in CRM. In fact, problem-solving can nicely be interpreted as question-answering within CRM. That is, to solve a problem means exactly to obtain the best answer to the problem.¹³ Indeed, it is exactly via CRM that not only can we render more precise the meaning of problem-solving but also effectively measure the performance of the corpus. However, as corpus performance is measured by the effectiveness of its question answering capacity, it is pertinent for us to clarify further what it means by corpus K being capable of sustaining answers to questions.¹⁴

As we all know, within the framework of CRM, the best answer to a given answer has to be one member from the set $M(M_e)$ ---the set of potential answers. $M(M_e)$ itself, however, is generated by $U(U_e)$. If K is in any sense responsible for providing answers to a given question, then K must be in some sense responsible for producing $U(U_e)$. In fact, insofar as K is seen as a standard for serious possibility, K is in a sense capable of determining those answers that are serious from those that are not. Indeed, Levi himself maintains that the set of serious answers is chiefly determined by X 's corpus at a particular time. But caution must be taken to distinguish between serious answers and relevant answers. Unlike serious answers, relevant answers are not determined by corpus K but by the problem X has. If this is the case, does it mean that K has no effect on determining the ultimate partition $U(U_e)$?

Indeed, if we interpret $U(U_e)$ literally as a set of relevant answers, then it is true that according to Levi's own idea, K has no role to play in determining ultimate partitions. However, it seems that the set of answers in a given context should not be seen only as relevant answers, but should be seen as relevant and serious answers. Hence, the members of $U(U_e)$ should be determined by both the corpus and the problem. Though Levi has not explicitly discussed this issue, I think that such an interpretation is not incompatible with his system. If this is the case, then it

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is clear that K is not in any sense directly offering answers to questions. But in delimiting the members of the ultimate partitions, it is instrumental, though in a more oblique way, in providing answers to questions. From this, it follows that given the same problem but using a different corpus a different ultimate partition presumably would be produced. There is another role for K . Ultimate partitions are ultimate partitions, that is to say, their elements are exhaustive and exclusive given K . Clearly, exclusivity and exhaustiveness are dependent typically on what is in K . Of course, the third role is as antecedent for the conditional probabilities in one's decision matrix, viz. $p(\text{outcome/act and } K)$. In this sense, the acceptance of different corpora will sustain different answers to a given question which in turn will affect the epistemic utility we finally obtain.

MEASURING CORPUS PERFORMANCE USING CRM

On basis of the above discussions, let us see how corpus performance is measured. Suppose a set of contracting strategies produces a corresponding set of corpora K_a, K_b, \dots, K_n . For simple illustration, let $n=2$. The way to measure the "performance" of the K 's is as follows:

Suppose we have a problem Q . The performance of K is measured by how well K can help in sustaining answers to Q .

Once again care must be taken here to realize that K cannot in any way directly provide answers to Q . In other words, it cannot be seen as a "box of answers" out of which answers are picked. In fact, K 's role in providing answers to Q is rather oblique. That is, as a standard for serious possibility, it helps to delimit the ultimate partitions relative to Q , and hence indirectly contributing to offering answers to Q . In this sense, the performance of K relative to Q can be seen as the epistemic utility of the best answer obliquely offered by K to Q . Suppose the epistemic utility of the best answer obliquely offered by K to Q is $EU(ans_{KQ})=v$, where v is a real number indicating the value of the epistemic utility of the answer, then K 's performance is measured in terms of the value v . The epistemic utility of answers here can be obtained by using Levi's rule (A).

Let us expand on the ideas just presented. Given a problem Q , there is a set of relevant answers $U(U_e)$, and a corresponding set of potential answers $M(M_e)$ generated by $U(U_e)$ relative to Q . Corresponding to every member of the set of potential answers, there is an epistemic utility attached to it. Among this set of answers only the best is chosen via rule (A) and its epistemic utility is taken into account. If there is only one problem, then the epistemic utility of the best answer relative to Q is taken as the performance of K . Let $PF(K)$ be the performance of K , then

$$PF(K) = EU(ans_{KQ}) \text{ ----- (a)}$$

However, suppose we have more than one problem and K is supposed to be capable of obliquely offering solutions to these problems. If we have n problems Q_1, Q_2, \dots, Q_n , then the performance of K can be obtained by the following formula:

$$PF(K) = \sum_{i=1}^n EU(ans_{KQ_i}) \text{ ----- (b)}$$

If the problems (questions) are of equal importance, the performance of K can be easily computed using formula (b). However, it is natural to assume that some problems (questions) are more important than others. Therefore, the importance of the problems has to be taken into account in the calculation as well. In view of this, we shall propose a scaling constant a, b, \dots etc to represent the relative importance of the problems. Interproblem scaling can be achieved by constructing interproblem scaling constants in the following way.

If Q_1 is given a value of , say, m on a given scale, Q_2 is given a value of n on another scale; we obtain the normalized scale of the Q 's as the ratio of the individual weights over the sum of the individual weights. In this case, the normalized weight of Q_1 is $n/(n+m)=w_1$, that of Q_2 is $m/(n+m) = w_2$. And it is easy to see that

$$\sum_{i=1}^n w_i = 1.$$

The w_i 's are regarded here as the interproblem scaling constants of the problems.

On basis of this deliberation, (b) should be modified as follows: •

$$PF(K) = \sum_{i=1}^n w_i EU(ans KQ_i) \quad \text{--- (c)}$$

By means of (c), the performance of other K 's, can be computed. Meanwhile, on basis of the performances of the K 's, the preferences over them can be determined accordingly. Let " $>$ " be "is preferred to" and " \sim " be "is equally preferable as", then

$$K > K' \text{ if and only if } PF(K) > PF(K') \quad \text{--- (e)}$$

$$K \sim K' \text{ if and only if } PF(K) = PF(K') \quad \text{--- (f)}$$

It is easy to see that this method of measuring corpus performance is equally effective in measuring the performances of mcs's. Consequently, using (e) and (f), the optimal mcs can be readily determined. This means that with respect to our decision-theoretic formulation of the Duhem problem, we have a ready solution to it.

Notice that our line of combating the Duhem problem is basically a pure Levian line. We take the technique of question-answering as the core of our strategy. K is evaluated in terms of the only information concept which Levi thinks plausible---informativeness of answers to questions locally defined. In sum, the epistemic performance of the corpus is understood here as a function of its question-answering capability.

CONCLUDING REMARKS

Let me summarize what we have done so far in this chapter. A decision-theoretic formulation of the Duhem problem is attempted within CRM. The Duhem problem is then transformed into a problem of optimal corpus contraction---how to contract an inconsistent corpus given an anomaly while retaining the maximal expected epistemic utilities. Specifically, our procedure have been to let the inconsistent corpus contract in order to form a set of maximally consistent sets, or mcs's. The next step is to choose the best mcs among the set of mcs's. This is accomplished by a way of measuring the performance of these mcs's which utilizes the Levian question-answering technique. With the help of such a measure, some preference rules are proposed as a basis for epistemic decision relative to the Duhem problem situation. As a result,

relative to our formulation of the the Duhem problem, we think that we have provided a solution to the Duhem problem.

Nevertheless, we do not claim that our solution to the Duhem problem is the only or the best solution. As a matter of fact, other non-decision-theoretic ways to tackle the problem are indeed available.¹⁶ And they may be equally plausible as well. To mention only one, for example, Lakatos, in his discussion of sophisticated methodological falsificationism, has in effect proposed some sort of solution to the Duhem problem. Interestingly, as we have noted before, our solution in effect seems very much the same as Lakatos'.

On the other hand, despite our criticisms, Dorling and Koertge seem to provide interesting suggestions with regard to the solution of the Duhem problem. The Duhem problem as formulated within CRM concerns only the situation where observation impinges on theoretical complexes which have already been accepted. That is, they are given probability 1. In both Dorling's and Koertge's cases, they do not require hypotheses to be accepted. That is why both the hypotheses under test and the auxiliary hypotheses are given probabilities less than unity. In this perspective, our version of the Duhem problem, in comparison with theirs, is a rather restricted one. Indeed, in an oversimplified way, our version can be seen as a "special case" of their versions.¹⁷ Moreover, Dorling's pure probabilistic approach

to the Duhem problem, as we have suggested before, might be a viable alternative as well. Be that as it may, however, what is interesting about our approach lies not so much in the solution it offers as in the way the problem is conceived and solved.

With reference to the basic ideas of Duhem presented at the beginning of our discussion, the idea that theoretical decisions can never be determined by empirical data alone is basically sound. Theoretical decisions are, to be sure, usually arrived at as a result of both the theoretical context (which includes theoretical commitments and the existence of rivals) and empirical evidence. Nevertheless, with regard to the claim that in cases of anomalies, investigators can have free choices in deciding what part(s) of the theory to be eliminated, we think that our discussion has shown that such a claim is not warranted at all.

As we have pointed out in our introduction, the Duhem problem, broadly construed, can be interpreted as a special case of the problem of induction. If this is the case, then clearly if we have a solution to the general problem of induction, the Duhem problem will be automatically solved. But the converse is certainly not true. Though we have provided an answer to the Duhem problem decision-theoretically interpreted, it is far from being an adequate solution to the general problem of induction. However, with Hempel,¹⁸ we believe that the

decision-theoretic approach to science in general, and to induction in particular, is a programme worth trying.

Finally, we have to note that even within Levi's CRM, the technique of measuring corpus performance requires further development. Recall that our measure of corpus performance is only restricted to two kinds of epistemic utility---truth and information. As we have pointed out repeatedly, explanatory power, generality, simplicity and so on should also be counted as important utilities relevant to scientific deliberations. It is then reasonable to expect that they be somehow included into the measurement as well. Incorporating these various utilities in the measurement undoubtedly would complicate the whole procedure. Nevertheless, in order to have a more adequate and comprehensive theory of epistemic decisions, this seems unavoidable. As a conjecture, I expect that a more complete account of epistemic decisions should be a multiattribute utility theory à la Keeney-Raiffa.¹⁹

FOOTNOTES

1. This in fact is a slight oversimplification. There may be a case where we have an incorrect removal of true elements to yield a consistent but false corpus.

2. Levi, EOK, p. 61-2.

3. The Duhem problem envisaged here is where H and A are within the corpus.

4. A recent paper by Goossens (1976) shows that Levi envisages experiments as choices which can be evaluated with respect to likely benefits for answering questions. In this sense, observation can be seen as obtained via deliberation.

5. I am indebted to Professor Nicholas on this argument.

6. This reflects the fact that competitors compete not by being inconsistent with h per se, but by being alternative answers to some question.

7. This conforms with condition (E) in chapter 6, which states that false answers, no matter how informative they are, should be less preferable to less informative true answers.

8. Levi: "From X's point of view, every item in his corpus is infallibly true and certainly true." [AR, p. 10] Also, "...once an item is incorporated into a corpus, its pedigree becomes irrelevant to its use in subsequent inquiries and deliberations." [AR, pp. 26-27]

9. This is the case when we have non-additivity of utility, i.e. $u(h \& k) \neq u(h) + u(k)$.

10. In contrast, Levi incidentally mentions that corrigibility has some connections with informational value. [EOK, p. 62]

11. All references to Laudan in this section are in Laudan [1977].

12. Laudan, ibid., p. 25.

13. Though accepting the disjunction of the elements of the ultimate partitions sometimes may be seen as accepting the best answer, this however is equivalent to abstaining from giving any solution.

14. The term "sustaining" was originally suggested to me by Professor Nicholas. It is used to indicate that K is not

treated here as some "box of answers", since the answers accepted as strongest via induction might not be in K.

15. In fact, $EU(ans_{KQ})$ is only a short-hand representation of the expected utility of the best answer sustained by K relative to some question Q, i.e.

$$EU(ans_{KQ}) = EU(h, e) \\ = p(h, e)u(C_h, t) + (1-p(h, e))u(C_h, f) \\ \text{or using Levi's formula,} \\ = p(h, e) + (1-p(h, e))(1-m(h, e)).$$

16. Other ways to solve the Duhem problem which offer the same generic solution are Lakatos [1970, section 2c] and John Nicholas [1978]. Indeed, even Dorling's way may prove to be a promising approach as well.

17. It sounds a bit strange to refer to our version as a special case of Dorling's and Koertge's, since in our case more conceptual resources (notably utility assignments) are invoked.

18. In commenting on the merits and weaknesses of the decision-theoretic approach to induction, Hempel expresses his guarded optimism as follows: "...I believe that there is something fundamentally right about the idea of epistemic value, and that the failure of the utility measure...may be attributable to a too narrow construal of the objectives of basic research." [1981. p.399]

19. cf. Keeney and Raiffa [1976]

APPENDIX A

Recall that we have

$$p(E', T) = p(E', T \& H) p(H) + p(E', T \& -H) p(-H) \text{----- (g)}$$

$$p(E', H) = p(E', T \& H) p(T) + p(E', -T \& H) p(-T) \text{----- (h)}$$

$$p(E') = p(E', T) p(T) + p(E', -T) p(-T) \text{----- (i)}$$

We need the values of $p(E', -T \& H)$, $p(E', -T)$ and $p(E', -T \& -H)$.

It is clear that

$$p(E', -T) = p(E', -T \& H) p(H) + p(E', -T \& -H) p(-H) \text{----- (j)}$$

In order to solve the problem, we need to know the value of $p(E', -T)$. By assigning subjective probabilities to hypotheses we get the values as follows:

$$p(E', T \& -H) = 0.05 \text{----- (k)}$$

$$p(E', -T \& H) = 0.001 \text{---- (l)}$$

$$p(E', -T \& -H) = 0.05 \text{--- (m)}$$

By substituting values, we get

$$p(E', -T) = 0.001 \times 0.6 + 0.05 \times 0.4 = 0.0206$$

$$p(E', T) = 0 \times 0.6 + 0.05 \times 0.4 = 0.02$$

$$p(E', H) = 0 \times 0.9 + 0.001 \times 0.1 = 0.0001$$

$$p(E') = 0.02 \times 0.9 + 0.0206 \times 0.1 = 0.02006$$

By substituting values once again, we finally get

$$p(T, E') = p(E', T) p(T) / p(E')$$

$$= 0.8976$$

$$p(H, E') = 0.003$$

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